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BASE-LINE APPARATUS.

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THE sources from which information has been drawn for this paper, are the reports of geodetical surveys of England, India, France, the United States, and several contributions to the American Philosophical Transactions. Besides these, Col. A. R. Clarke has given a sketch of the subject in the article entitled Geodesy in the Encyclopedia Brittannica, and also a more extended review in his recent work upon Geodesy. Further than these there appears to have been no treatment of the matter as a whole, probably because there is greater interest attached to the larger fields of general geodetical research.

The degree of accuracy with which angles are measured by such instruments as those of Würdemann, Ramsden, and others, compels a corresponding degree of precision in the measurement of base lines. But though an angle may be easily measured and remeasured until theoretically and practically a very high degree of accuracy is attained, the repetition of the measurement of a base line requires an outlay of time and money that becomes a matter for serious consideration. The length of a measuring bar being once determined, it is evident that any error in its supposed length or in the method of using it will be repeated as many times as it is used in measuring the base, and hence no pains should be spared to secure the highest possible degree of exactness in its construction and use. The apparatus should also be light, portable, and easy of manipulation.

The measuring bar must be of known length, and its variations from a standard length must be rigidly determined as regards their amount and regularity. In connecting two systems of triangulation the units of length employed in each must be compared. Hence it is that such comparisons become of primary importance, and the first portion of this paper will be devoted to that subject.

In comparing the length of one bar with another or with standards of length the bar is usually placed horizontally. The manner in which it is supported will require attention, since the bar will be deflected by its own weight, and consequently shortened horizontally. The following is an investigation of the change in length due to deflection as given by Clarke, somewhat expanded. Let a be the length of a rectangular bar

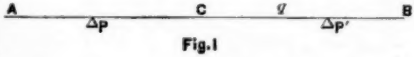


Fig. I

of depth k and width h. Let w be its weight and d the total extension of the

bar due to a load w attached to its lower extremity when suspended vertically. Suppose AB to be the bar, supported horizontally at the points P, P′, whose distances from the center C are b and b' respectively. If E denote the coefficient of elasticity, then

$$E=\frac{w}{hk}\div\frac{d}{a}=\frac{wa}{dhk}.$$

The moment of resistance to flexure is

$$M=\frac{EI}{r}=\frac{E}{r}\cdot\frac{hk^3}{12}=\frac{wak^2}{12dr},$$

in which I represents the moment of inertia and r the radius of curvature at any point as g. Using rectangular co-ordinates, the origin being at C, and the axis of x passing through the points P and P′, the moment at any point between C and P′ is

$$\frac{wak^2}{12dr}=\frac{bw}{b+b'}(b'-x)-\frac{w}{2a}(\tfrac{1}{2}a-x)^2; \quad . \quad (1).$$

and between P′ and B is

$$\frac{wak^2}{12dr}=-\frac{w}{a}\left(\frac{a}{2}-x\right) \quad . \quad . \quad . \quad (2).$$

If in (1) $\frac{1}{r}=\frac{d^2y}{dx^2}$ be equated to zero and the equation solved for x the resulting value of x will be that of the point of inflection. Thus,

$$x=\tfrac{1}{2}a\frac{b'-b}{b'+b}\pm\frac{1}{b'+b}\sqrt{abb'}.\sqrt{2(b'+b)-a},$$

from which it is evident that a real point of inflection is only possible when

$$b+b'\geq\frac{a}{2}.$$

The shortening of the upper fiber will be

$$\tfrac{1}{2}k\int_0^b\frac{d^2y}{dx^2}dx+\tfrac{1}{2}k\int_b^{\frac{a}{2}}\frac{d^2y}{dx^2}dx$$

$$+\tfrac{1}{2}k\int_0^{b'}\frac{d^2y}{dx^2}dx+\tfrac{1}{2}k\int_{b'}^{\frac{a}{2}}\frac{d^2y}{dx^2}dx$$

$$=\frac{3d}{ak}\left(bb'-\frac{a^2}{12}\right).$$

If this extreme fiber is to retain its original length bb' must equal $\frac{a^2}{12}$, or $b=b'=\frac{a}{2\sqrt{3}}$ is the condition for a bar supported symmetrically. When, however, a bar is supported at distances from one extremity equal to $\frac{1}{4}$ and $\frac{3}{4}$ its length, as is often the case, the horizontal projection of the upper fiber will be less than the actual length by $\frac{1}{16}\frac{ad}{k}$.

Before the discovery of this theorem by Airy, the British Ordnance Survey found the error due to deflection by laying a straight-edge upon the bar and measuring the deflection by inserting a wedge between the bar and straight-edge. If the curve of the neutral axis be considered a circle, the length of the required chord subtending it is readily calculated from the deflection.

The effects of flexure may be overcome in several ways; as by floating the bar in mercury either loaded with weights or not; or by cutting down until the neutral axis is exposed, and marking the extremities of the measure upon it. By this latter method any error due to tension or compression of fibers is obviated, but not that due to curvature.

Standards of length, with which bars are compared, may be divided into two general classes: Standards "à bouts," in which the ends of the bar are disk-shaped; and standards "à traits," in which the length of the bar is indicated by lines or dots engraved on the neutral axis. In the first class an error may arise, when a microscope is used in making the comparisons, by sighting at a point on the disk which is not at the extremity of the axis. Clarke has shown the probable error to be a minimum when the radius of curvature of each disk is equal to the length of the bar.

The thermometer with which the temperatures are taken during these comparisons must be of superior workmanship, and more especially is this true of those which are to serve as standards with which to compare other thermometers used in the field or elsewhere. The index and calibration errors must accordingly be determined at intervals in order to discover any changes which the thermometer may have undergone. Thermometers may be compared at high temperatures by immersing them in hot water and making comparisons as the

water cools; but for lower temperatures it is probable that a somewhat greater degree of accuracy is obtainable by readings taken when the temperature is nearly stationary and the thermometers in a protected place.

The comparison of bars is usually conducted in a structure erected especially for the purpose. The British Ordnance Survey building in which this work is conducted is a room half sunk in the ground, roofed over with nine inches of concrete, and having double walls. It is completely surrounded by an outer building, and thus the changes of temperature are of the most gradual character. Three stone piers built upon deep brick foundations rise through the flooring, but have no connection with it. Upon them rest heavy cast-iron blocks which hold the microscopes in position. The comparisons are made in the following manner: The bars are each placed in two rollers in a box, and leveled by means of a vertical movement imparted to the rollers. One of the bars is then brought under the micrometers and readings taken, the temperature being noted at the same time. The first bar is then replaced by the second and the micrometers adjusted and read, then thrown out of focus, readjusted and again read. Finally, the first bar is put under the microscope and observed as was the second, after which the temperature is taken.

It is to be noticed that the temperature of any body as indicated by a thermometer cannot be correct unless the body either possesses the same specific heat, absorptive, radiant, and conductive powers as mercury, or the temperature is stationary; and hence all observations made in the field during the measurement of a base line are subject to an error of which account should be taken.

The errors of the micrometers and the personal errors of the observer are also matters to be considered. In the series of comparisons made by the Ordnance Survey between 1831 and 1842, it was discovered that the stone pillars then used had sufficient motion to produce an error. This difficulty has probably since been overcome.

As illustrating the method by which bars are reduced to the standard temperature, the following is taken from Yolland's Ordnance Survey. Suppose two bars, A and B, are to be compared.

Let a, a_1, a_2, &c., denote the observed differences of length;

m, m_1, m_2, &c., the differences between the observed temperatures of A and 62° Fahrenheit, which is the standard temperature adopted;

n, n_1, n_2, &c., the same differences for B; x, y, the rates of expansion of A and B respectively for each degree Fahrenheit;

z, the true difference of length of the bars at 62°.

The observations will then furnish a series of equations; as,

$$a+mx-ny-z=o,$$
$$a_1+m_1x-n_1y-z=o,$$
&c., &c.

By the method of least squares the following normal equations may be formed:

$$\Sigma am+x\Sigma m^2-y\Sigma mn-z\Sigma m=o,$$
$$-\Sigma an-x\Sigma mn+y\Sigma n^2-z\Sigma n=o,$$
$$-\Sigma a-x\Sigma m+y\Sigma n+pz=o,$$

when p denotes the number of observations. The most probable values of x, y, and z are therefore known.

In the comparisons made by the Coast Survey Saxton's pyrometer is employed instead of the micrometers, and it is quite certain that the results are thereby rendered more trustworthy. This instrument may be briefly described as follows. The bar under inspection is allowed to expand at one end only, and in so doing pushes a sliding rod to which is attached a very delicate chain. The latter by being unwound communicates the motion of the rod to a cylinder, causing it to revolve together with an attached mirror. At some distance is placed an arc, and to the rear of and above it a telescope. The mirror reflects the graduations of the arc into the telescope. A very slight motion of the mirror will cause a considerable change in the reading of the arc. A full account of the method in use by the Coast Survey is contained in the report for 1862, from which the above description is taken.

The Ordnance Survey building above described is supplied with apparatus for determining the absolute rates of expan-

sion of standards. The bars are placed in tanks, one of which contains ice water and the other hot water supplied to it by means of flexible pipes leading to the supply which is kept without the building. The tanks are so arranged that they may be placed under the microscopes fixed upon the piers without re moving them from the tanks. A complete observation consists in comparing a bar in the hot tank with another in the cold tank, and then making a similar comparison after interchanging the bars in the tanks.

A very neat arrangement has been used by the Ordnance Survey to insure the parallelism of the surfaces of two bars when brought successively under the microscopes. It is simply a curved lever, the short arm of which carries a hook or point which rests on the bar, while the longer arm traverses a vertical scale. It is only necessary after having leveled the first bar and recorded the readings of the levers at each of its extremities, to make the readings of the levers for the second bar agree with those of the first by means of the leveling screws under the bar. By this arrangement any error due to a want of parallelism of the surfaces observed, or of the axis of the microscopes, is wholly overcome.

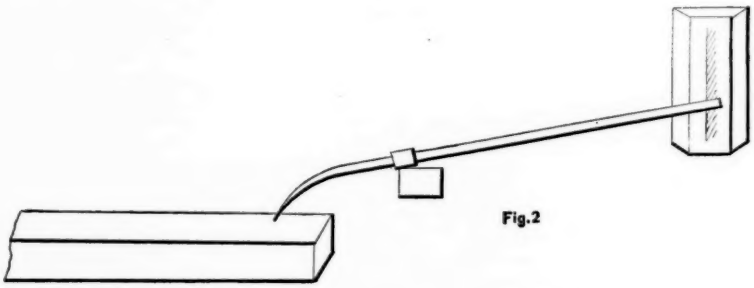


Fig.2

The first base lines were measured with rather crude devices. The rods used by the expedition of the French Academy at Tornea, in 1736, were of fir, each five toises in length. A toise is about six feet.

The base at Hounslow Heath was first measured with deal rods terminating in bell-metal tips; but the inaccuracy of these became so apparent that they were abandoned and glass tubes 20 feet in length were substituted. This measurement was afterwards verified by using a 200 foot chain, constructed by Ramsden, which was laid in deal coffers supported by wooden trestles, and stretched by a weight of 28 pounds.

A great impetus was given geodetical operations by the determination of the length of the meter, wich is a ten-millionth of a quadrant of the earth. The necessary triangulation for this purpose was undertaken by the Constituent Assembly of France in 1792.

In 1827 Colby began the trigonometrical survey of Ireland by the measurement of a base near Londonderry, with an apparatus the fundamental principle of which was that of compensation as in the gridiron pendulum. This principle has been employed in the construction of all the more accurate instruments of this character in the United States.

Suppose two rods, bb' and ii', to be fixed at their centers, o and o'. If at some temperature they are of equal length, let that temperature be increased until ob' expands to ob'', and $o'i'$ to $o'i''$. Should a strip of metal be fixed across the bars in the position $b'c$, it is evident that if the strip be so pivoted to the bars that $b'c : i'c :: e_b : e_i$, where e_b and e_i represent the respective rates of expansion of the bars, then the point c will not vary its distance from o''. Thus, if a point be similarly fixed at the left-hand end of the rods its distance from c will be invariable provided the rates of expansion are constant, and the rods do not at any time differ from each other in temperature. The rates of variation in temperature will be due to difference in mass, conductivity, powers

of radiation and absorption, and specific heat.

It is known that if a bar be heated and then cooled to its original temperature, it does not necessarily return to its original length. The principle of compensation will no doubt be adandoned in time for the more accurate method of the Spanish and Algerian surveys, to be hereafter described. It is doubtful whether Colby grasped the problem in its entirety, though he adopted such means as would correct the errors due to the factors above enumerated. He chose iron and brass as his materials; the former he decided to make the compensating material. In order that they may be of the same temperature, rods should acquire equal increments of temperature in the same time—that is, their absorbtive powers should be equal. This may be accomplished by properly adjusting the character and relative area of the surfaces of the rods. Colby coated the iron bar with a mixture of varnish and lampblack, gradually removing it from portions of the surface until, by experiment, the required adjustment was effected. This coating was then removed and a new one applied containing the requisite quantity of lampblack. The Colby bars rested upon rollers at $\frac{1}{4}$ and $\frac{3}{4}$ their length, and were connected at their centers by a pair of cylinders. The tongue was of steel, carrying a silver pin at the outer extremity, upon which the compensated dot was placed. The whole apparatus was inclosed in a wooden box from which nozzles projected at each end to serve as protectors for the tongues. A lid in each nozzle permitted the observation of each dot by means of a microscope. Pins passed through the cylinders connecting the bars, and were inserted in the sides of the box to prevent lateral motion. In the top of the box was an aperture, closed by a lid, through which a level atched to the brass rod could be viewed. A vane-sight was screwed to each end of the box to serve in alignment. The apparatus was supported upon an arrangement technically known as a "camel," at $\frac{1}{4}$ and $\frac{3}{4}$ its length. These devices provided for a horizontal as well as a vertical motion, and were in short the means of aligning and leveling the box. The trestles used by Colby were of wood, and

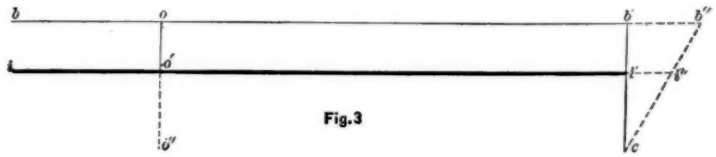


Fig.3

not of elegant design, though very substantial. A plate firmly screwed to the end of each box served as a support for a three-armed grooved stand upon which was placed the compensating microscope. Each box with its plates weighed 136 pounds.

The compensating microscope consisted of three microscopes. Two were held in position at such a distance as to keep their foci six inches apart by means of arms projecting horizontally from collars which encircled the central microscope near its upper and lower ends. These bars, being made of brass and iron, acted as compensators. The outer

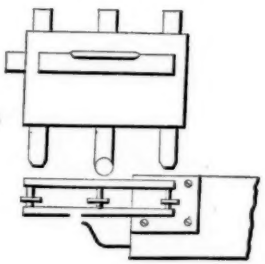


Fig.4

microscopes had a focal length of two inches. The central or telescopic microscope had its focal distance varied by means of a screw projecting horizontally. The three were inclosed in a rectangular box which was supported upon a cylinder surrounding the central microscope.

This cylinder was attached to a plate which could be put in motion horizontally by means of tangent screws. On opposite sides of the rectangular box were attached a level and a telescope for alignment. The weight of the microscope was 5 pounds.

The telescopic microscope transferred the terminal point vertically to an arrangement known as a "point carrier," which served to fix the end of a day's work, or answered some similar purpose. It consisted of a heavy iron plate which carried a disk, or of an upright cylinder whose upper surface formed the disk. Upon this surface was engraved the line or dot which indicated the extremity of the measured distance, the disk being movable in a groove of the plate.

Colby's apparatus is still used in the English surveys, but does not appear to give entire satisfaction. In the measurements at Cape Comorin, during the triangulation of India, thermometers were used, and the base, which is nearly north and south, was divided into four segments, each of which was measured four times—twice with the brass bar to the east, and twice with the iron bar east.

In 1816 the Russian government undertook the trigonometrical survey of the provinces of Lithunia and Livonia. The latter survey was accomplished under the direction of the elder Struve; the former, by Tenner. The character of the country was so favorable that it was decided to take advantage of it in measuring the great arc, which extends from Ismail, near the mouth of the Danube, to the northern boundary of Sweden, a distance of 1,800 miles, and corresponding to 25° 20′ of arc. The task was completed in thirty-six years. It required the measurement of 10 bases; the determination of latitude at 13 points; and the location of 275 principal stations.

Struve invented a base-line apparatus, which may be briefly described as follows. It consists of a single bar of iron two toises long, terminated at one extremity by a small cylinder, while to the other extremity is affixed a lever, known as the lever of contact. The end of the short arm of this lever is spherical in form; the longer carries an index moving in front of a graduated arc attached to the bar. The reading of the arc indicates the length of the bar as found by observation. The lever, being placed in contact with the forward bar, is maintained in position by a spring attached

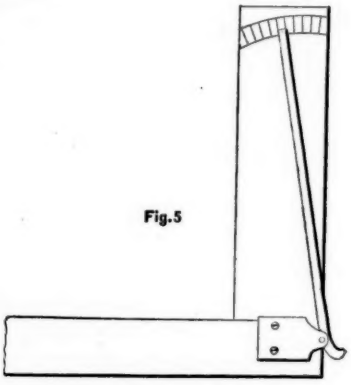


Fig.5

to the lever. A pair of thermometers lying in the bar indicate its temperature. The bar is wrapped with cotton and cloth to guard against rapid changes of temperature.

In the geodetical operations of Delambre, executed under the direction of the French Academy, Borda's apparatus was employed. Each rod consisted of a platinum strip two toises long, upon which lay a copper strip, free to expand in one direction only. The copper strip being somewhat the shorter, served as a measurer of the platinum strip. In practice this was effected by means of a scale engraved upon the copper, which was read by a vernier on the platinum. From this reading the length of the platinum strip was calculated. At the extremity of the platinum strip was a smaller piece of the same material, sliding in a groove cut in the larger strip, and having upon it a vernier, which served to measure the distance between successive bars. Both verniers were read by microscopes. The inclination of the rod was read from a vertical arc of two feet radius, whose error was eliminated by readings taken in reverse position.

Bessel's apparatus was similar to Borda's, with the exception of the device for measuring the intervals, and the substitution of iron and zinc for platinmu and

copper The intervals are measured by a scale cut upon a glass wedge, which is introduced between the bars. The zinc strip carries at each end a horizontal knife-edge, and the small strip of iron has two vertical knife-edges. The distance between the inner of these latter and the horizontal knife-edge of the zinc is measured by inserting the wedge. Let i denote the actual distance measured by the wedge; then if l_1 and l_2 denote the length of the strips at the time i is observed, we shall have:

$$i = z + m\,(x - y);$$

using the notation previously adopted. If l'_1 and l'_2 are the lengths at 62°, obtained by comparison with a standard;

$$l_2 = \frac{l'_2 x - l'_1 y}{x - y} - \frac{i\,y}{x - y},$$

which may be taken to be the length of the iron piece at any observation.

Bessel used four rods in his measurements, each similar to that above described. Represent the lengths of the iron pieces by L_1, L_2, L_3, and L_4. Let there be some length s, obtained by a comparison of one of the bars with a standard, and let v_1, v_2, v_3, v_4 denote small variations of length of the bars from s, so that $v_1 + v_2 + v_3 + v_4 = o$.

Also let t_1, t_2, t_3, t_4 be the observed temperatures, and r_1, r_2, r_3, r_4 rates of expansion. Then

$$\begin{aligned} L_1 &= s + v_1 + t_1 r_1, \\ L_2 &= s + v_2 + t_2 r_2, \\ L_3 &= s + v_3 + t_3 r_3, \\ L_4 &= s + v_4 + t_4 r_4. \end{aligned}$$

From the eight equations obtained by a comparison of the rods *inter se*, and the condition $v_1 + v_2 + v_3 + v_4 = o$, the values of r_1, r_2, r_3, r_4, and v_1, v_2, v_3, v_4 are obtained, and the length of the base may be represented by an equation of the form

$$ns + av_1 + \beta v_2 + \gamma v_3 + \delta v_4 + a' r_1 + \beta' r_2 + \gamma' r_3 + \delta' r_4.$$

It may be seen by a comparison that four rods is the least number by means of which the unkown quantities can be determined.

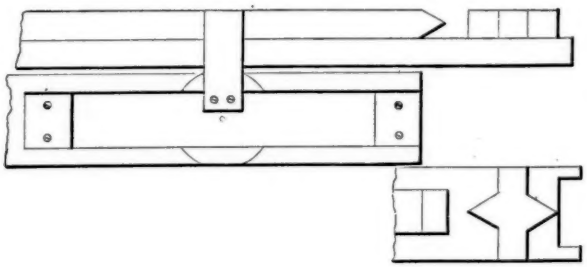


Fig. 6

In marking the close of a day's work Struve projected the terminal point on a cube sliding in a groove cut in an iron plate, by means of a transit set up at right angles to the line of the base. Bessel used a plummet to transfer vertically. In the Belgian bases, where Bessel's apparatus was used, a plate carrying a horizontal knife-edge at the rear end and a vertical one at the advanced end, served to indicate the end and beginning of operations, the distance between the knife-edges being part of the base. This plate moved in a groove cut in its support and could be clamped. Its iron support was built in with brickwork at some point previously determined upon.

The instruments mentioned above are, with one exception, those with which the principal European bases have been measured.

In this country the first base used in triangulation was measured in 1830 by Simeon Borden, Superintendent of the Massachusetts State Survey. His apparatus was constructed upon the compensation principle. Borden made his own apparatus and measured with it a base near Northampton, of 7.4 miles, with a probable error of 0.237 inches. The apparatus was contained in a tin tube, 50 feet in length and 8¼ inches in diameter, tapering toward the extremities. The tube was closed at its ends by cast-iron

plates through which the rods projected. These latter were of brass and steel, $\frac{3}{8}$ inch in diameter, and rested upon 19 supports. Each rod consisted of four segments which were united by means of mortises held in a "coupling-box." The rods were kept at a constant tension by a spring at one end of the tube which was compressed between diaphragms, the inner one being fixed and the outer pressed against an iron nut screwed upon a rod. This rod in turn pressed an arm attached to the brass and steel rods at equal distances from the iron rod. The couplings were fastened by movable joints to the arms or indices at each end, and the index not connected with the iron tension rod is made to stand at a constant angle with the axis of the tube by means of a stirrup-like arrangement screwed to this index and to the iron plate closing the tube. The compensated point was adjusted by means of two silver indices, one attached near the end of the arm and the other to the head of a clamp which could be regulated. The microscopes were compound, consisting of a single object-glass and an eye-picee of two lenses. They were held in frames supported by a trestle. The whole arrangement was evidently an adaptation of Colby's apparatus. Borden secured uniform absorption in the usual manner, but for some reason appears to have attempted no further adjustment of the rods for temperature.

The first base measured by the Coast Survey was under the direction of F. R. Hassler, the first superintendent, and is known as the Fire Island Base. The apparatus was of his own designing, and consisted of four two-meter bars inclosed in a wooden box. A single microscope read the index on successive bars. The base was $8\frac{3}{4}$ miles, and the probable error as given in the Coast Survey Report for 1865 is shown to be ± 0.0585 m. The apparatus now used by the Coast Survey is the invention of Bache, and in its construction involves the principles employed by Colby, Struve, and Borda. A very readable description of the instrument is given in VAN NOSTRAND'S MAGAZINE of 1875. Its general design may be sketched as follows: Two bars, each about six meters long, are contained within a double tube, coated white without, so that changes of temperature are very gradual and the annoyance arising from the use of tents is avoided. The bars are of iron and brass firmly united at one extremity. The iron bar is placed above and runs on the brass bar by means of stirrups and rollers. The lower or brass bar expands on rollers attached to the framework of the tube. At the free end of the bars is a curved lever pivoted to the lower bar and carrying upon its inner surface a knife-edge which is in contact with a steel plane attached to the inner bar. Fastened to the upper surface of the iron bar is a frame, through which slides a rod. The compensation lever passes into a collar carried by this rod, and its point abuts against one of the faces of the collar. A spring attached to the rod and frame in which it slides serves to press the lever back at a constant pressure, and consequently to cause a constant pressure between the knife-edge and the steel plane carried by the iron bar. The sliding rod has at its outer extremity an agate plane which is thus kept at a constant distance from the fixed extremities of the bars. The extremity of the apparatus just described is termed the compensation end.

The most important parts of the "sector end" may be described as follows. A sliding rod projects, which, coming in contact with the agate plane of the compensation end causes a pressure. It is necessary that this pressure of contact should be constant, and this has been secured by means of an arm pivoted to the lower bar, and against which the sliding rod abuts. At its upper end the arm presses a short tail which drops from a spirit level mounted on trunnions, so that it always requires the same force to bring the bubble to the center. This level is fixed to the sector proper, which is an arm, carrying at its inner extremity a vernier which reads a fixed vertical arc, whose zero corresponds to the central position of the bubble of a second spirit level attached to the sector. The axis of the level being parallel to the axes of the bars, the arc reading indicates the inclination of the apparatus to the horizon, from which the length of the horizontal distance corresponding to the measured length is readily deduced. The trestles which support the apparatus are of careful design, and by means of the horizontal screws and of a rack-and-

pinion movement of the legs considerable latitude is attained. In measuring a base, wooden frames are approximately

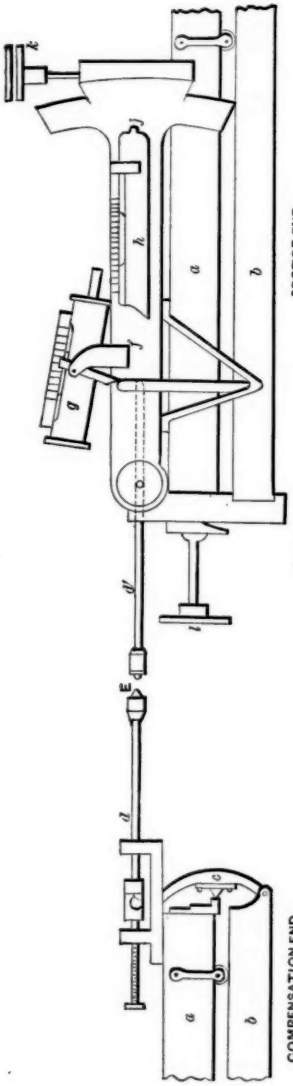


Fig. 7

a, a, The iron rod. *b, b*, Brass rod. *c*, Lever of compensation. *d, d'*, Sliding rods. *E*, Agate surface. *f*, Lever of contact. *l*, Screw to bring rods together. *h*, Level attached to scale *j*, which indicates the slope. *g*, Level of contact. *k*, Screw to level *h*.

adjusted in advance for the trestles to be placed upon.

In the construction of the bars, Bache not only used the device of Colby for insuring equal temperatures of the rods, but made allowance for different conducting powers, and adjusted their masses inversely as their specific heats. There appears to be a permanent change of length of the bars which is probably irremediable. It is the result of changes of temperature.

The measuring bars are compared with the standards of length both before and after the completion of a day's work. For this purpose a modified form of Saxton's pyrometer is used, in which the bar is made to abut against a horizontal arm which projects from the vertical axis of the mirror. The first base measured with this form of apparatus was at Dauphine Island, near Mobile, in 1847. The base was seven miles in length, and required seventeen days for its measurement. In the rapidity with which the operations are executed Bache's apparatus is superior to any other, 1.06 miles having been measured in a single day The tests to which it has been put certainly show it to be superior to all except the base line apparatus of Porro, which will now be described.

This method differs considerably from those before employed, for but a single bar is used which serves to measure the distance between successive microscopes placed upon fixed stands. The bar consists of two cylindrical rods, united as in Colby's apparatus. A strong deal box protects the rods, which extend beyond the box at each extremity, and carry a fine scale. The microscope is so arranged that a point on the ground several feet away can be read as well as one on the rod at the distance of a few inches. This is accomplished by means of an object-glass of long focal distance, in the center of which is inserted another of short focal length. Beneath the microscope is an adjustable screen, so perforated as to permit the use of the smaller glass when the light is cut off from the larger object-glass.

The microscope is held in position by two rings attached to horizontal arms projecting from a vertical cylinder. The cylinder is supported by a stand which rests upon levelling screws. By the aid of these screws and of an attached level the microscope may be rendered vertical. The microscope is capable of a slight vertical movement, whereby the focal

adjustment is perfected. On the cylinder opposite the microscope, is a telescope held by a bracket, which serves as a counterpoise to the microscope. A graduated scale may be substituted for the telescope, and when read by the telescope of the preceding microscope stand in connection with a signal set up on the line of the base in advance, it serves to indicate the direction of the bar. The telescope is constructed for making these simultaneous readings in a manner similar to that employed in the microscope. It consists of a small lens placed in the tube and capable of such motion by means of a rack and pinion as to bring the scale to view in the same field with the advanced signal. The use of a double object-glass in base-line apparatus is probably due to Haswell, though he employed the device in a crude form, He made the two halves of the object-glass of his microscope of glasses of different focal length. Apparently Porro's apparatus is superior to those before described, and actual use has shown this to be a fact.

The base is measured by placing four microscopes on trestles, approximately aligned at a distance of three meters apart. The single measuring bar is then transferred between successive microscopes and the scales read. This instrument, as improved by Ibanez, has been used in the Spanish survey, a platinum rod being substituted for steel.

For the measurement of secondary bases, either as a verification or as preceding the primary triangulation, it is necessary to have an apparatus which shall be of easy manipulation, light and durable construction, and shall, without an over nicety, be capable of comparative exactness. For this purpose an apparatus was constructed by Hilgard and others of the Coast Survey, which is described in the Coast Survey Reports of 1856–57. It consists of a single rod encased in a wooden box. The temperatures are read by means of inserted thermometers, and by means of a spring arrangement contacts are rendered quite accurate. The trestles permit of considerable vertical motion, which is obviously of great importance over a comparatively rough base.

Despite the exceeding delicacy of base-line apparatus, it still admits of improvement. If some method can be found to eliminate the error arising from unequal rates of expansion and contraction, and if permanent changes of length can be obviated, something of importance will have been gained.

Bache's apparatus has been very thoroughly tested in the measurement of a base at Atlanta, twice in winter and twice in summer; the probable error being $\pm 1.76u$,– u, denoting one millionth of the length measured. The probable error of Colby's apparatus is stated as $\pm 1.5u$. Struve has placed the error of his base as $\pm 0.8u$, which Clarke regards as incorrect. The error of Bessel's apparatus for a base of $2488m$. $\pm$is $0.59u$; For Porro's measurement the error is ± 0.32; for the base at Madridijos, $14664.5m$., it is estimated at $\pm 0.17u$.

Comparisons might be made of the relative accuracy of different constructions by means of the probable errors of base measured. But it is obviously not a just method, since the length of the base is directly proportional to probability for error, and the probable error is some function of the temperature as regards amount of change and rapidity of its variations. The number of bases measured with any form of apparatus being but small, it is evident that the influence which the probable error, obtained by the measurement of an additional base upon the average probable error of the instrument, will be considerable. If such comparisons be made, the final probable error of a single apparatus should be determined by a comparison of the probable errors of the bases which it has measured, the range of temperature and length of base entering as weights, and the probable errors thus obtained should be compared upon a similar basis.

Such a comparison, however interesting theoretically, is practically unimportant, since it has been shown conclusively, that Bache's apparatus is unapproached for the ease and rapidity with which it may be manipulated, though perhaps slightly inferior to Porro's in accuracy.

No fewer than two German expeditions will come to this country to observe the transit of Venus next year.

University of Michigan

BLASTING UNDER WATER.

By J. DEUTSCH.

From "Wochenschrift des Oesterreichischen Ingenieur-und Architekten Vereine," for Abstracts of Institution of Civil Engineers.

The author, as delegate of the Austrian Engineers and Architects Association, attended the experiments conducted by Major Lauer before a commission, appointed by the Imperial Minister for War, to report upon his method of blasting under water, by means of a charge laid upon the surface of the mass to be operated on, and fired by electricity.

For carrying out this operation an ordinary river flat or barge is employed; over the stern two beams are rigged out, in which a couple of uprights, connected at the top by a cross beam, are fixed; in the center of this cross beam there is an iron stirrup; the uprights are further strengthened and stayed by a couple of longitudinal ties.

A movable grating forms the floor of this overhanging stage at its extremity, and across its entire width there is a row of apertures through which the sounding-rod works, after passing through the stirrup on the cross beam, and which together regulate the position and direction it is desired the rod shall assume.

The rod itself is made up of several lengths of 1½-inch gas-pipe, each length being fitted at one end with a solid iron mandril, at the other with a strong coupling. A chain attached to its lower extremity enables it to be lowered or raised by hand from the deck of the flat. This arrangement permits of the rod being adjusted in almost any position, and so as to reach any point within a circle of considerable area at the bottom of the water. The soundings, however, are all taken at an angle which by a simple calculation gives the true vertical depth, all necessary data being known. The depth may vary without being perceived and alter the angle, which might have the effect of changing the position of the blast; on this point the jury expressed preference for a system of vertical rather than of angular soundings. For the purpose of these experiments a mass of gneiss traversed by veins of quartz was selected, situated in the bed of the Danube near Kreus, and at a depth varying from 9 to 11 feet, the surface velocity being 10½ feet. The experiment occupied nine consecutive days, or six hundred and six working hours, and gave an average performance per day of ten hours of three hundred and fifty soundings and seventy-two blasts, each sounding occupying twenty-five seconds, and each shot from four to five minutes; the rest of the time was spent in altering the position of the barge. The total number of shots fired was three hundred and ninety-nine, on which 294 lbs. of dynamite were expended, and 43 cubic yards of rock removed. The force of the current washed away the débris, and the mass thus removed was ascertained by soundings taken shortly after each explosion; had this been practicable later it is probable greater results would have been recorded. The cost per cubic meter was found to be 12 gulden, 6 per cent. less than it has been estimated similar work at the Iron Gate, performed in the ordinary way, has cost.

A comparison of the system commonly adopted and that recommended by Major Lauer shows that the distinctive features of the latter do not so much lie in the fact that the charge is simply laid upon the object to be operated upon, without drilling or loading a hole, but rather in the ease and rapidity with which the charge is laid, and the precision with which the operations of sounding and blasting can be conducted. Besides, it must be remembered that the very obstacles which render the present system tedious and expensive, viz., great depth of water and strong currents, actually contribute to the economic success of the Lauer system, which puts blasting under water almost on the same footing as blasting on land.

The cost of blasting operations generally, whether above or below water, depends on the structure of the rock rather than its hardness; and the local peculiarities in each case, whatever they may be on land, are certainly much exaggerated

when encountered under water, where the sense of sight is inoperative, and that of feeling, mechanically supplemented by the sounding rod, alone available. Under the present system, especially where the water is deep and the stream rapid, the operation of drilling the hole is attended with uncertainity and great difficulty, and, if during the process the water vary considerably in depth, a satisfactory completion of the hole is almost impossible. This, together with the expense of the staging required, and the time occupied in removing and replacing it before and after each explosion, and preventing the bore-hole silting up, contribute to make the present system, even under the most favorable circumstances, a most expensive one; so that, even before the invention of dynamite, the plan of depositing free charges of gunpowder on the surface was resorted to in the years 1858–60 for blasting operations in the harbor of New York, with favorable results.

With dynamite the same system was further employed on the coast of Dalmatia, but with unsatisfactory results due probably to local peculiarities.

WIND MEASUREMENTS.

From "Nature."

Since the time of Hooke the accurate measurement of the wind has formed an object of experimental research. That philosopher, if not actually the first to invent an anemometer, at any rate appears to have been the first to write upon the subject, which since then has occupied the attention and exercised the ingenuity of many scientific men. The main result of these efforts was well shown last week at the exhibition of anemometers organized by the Meteorological Society. The President, in an interesting historical address, stated that the number which had been invented was at least one hundred and fifty, and upwards of forty of these were collected, besides photographs and drawings of many others. The exhibition was by kind permission held in the library of the Institution of Civil Engineers, at whose weekly meeting two papers, on the design of structures to resist wind, and the resistance of viaducts to gusts of wind, were very opportunely read.

It is not by any means generally recognized that there are two distinct objects for which the measurement of the wind is necessary; these are: (1) the determination of the actual motion or transference of the air itself; (2) the investigation of the effect of the wind. The two societies above mentioned well represent these two objects of anemometry, and all the instruments are included in one or other of the two classes, which are said to measure respectively the velocity and pressure of the wind. These terms, though convenient, are slightly misleading, as it is really the impulse of the wind which is in both cases measured —in one by its effect in producing the continuous rotation of a vane or set of cups, in the other by its statical effect upon a pressure board or column of air or liquid.

From the nature of the wind it is evident that nothing less than a continuous graphic record could be of much service, and but little progress was made until the invention, about fifty years ago, of self-recording instruments of both classes. The late Dr. Robinson, F.R.S., contributed more than any one else to the establishment of the velocity anemometer which, by the addition of Mr. Beckley's self-recording apparatus, is undoubtedly a model of mechanical invention. Mr. Follet Osler, F.R.S., as the result of much persevering labor and skill, has given to the world a pressure instrument of great excellence, and of this and the former, both of which may be regarded as the best types of the two classes, it may fairly be said that much improvement, at any rate in mechanical construction, can hardly be expected.

As to the tabulation of results, this is conducted with the most scrupulous regularity. Since 1874 the Meteorological Office has published hourly numerical records, from its various stations, of the direction and other elements of the wind. Quarterly records containing engravings

of the actual curves are also published. These latter have rather fallen into arrears, the first volume of the new series for 1876 having been only published in 1881; but it is satisfactory to hear that the work of completing them up to the year 1880 is progressing, and it is to be hoped that they will always be continued.

In the face of all this expenditure of time and skill the meteorologist and the engineer alike proclaim the unsatisfactory state of the science. The engineering aspect of the question, viz., the effect of the wind, has recently excited considerable attention in consequence of the Tay Bridge disaster in this country, and of similar accidents abroad. It is evident that with the increase in the size of engineering structures, particularly in exposed situations, the force of the wind may become as great as that impressed upon the structure by the action of gravity. The recent account, in this paper, of the proposed new Forth Bridge, was a good example of the provision made for wind pressure, not only on the completed structure, but also during its construction. Notwithstanding this, the report of the recent Commission on Wind Pressure substantiates the statements already alluded to. This distribution of wind pressure over any surface appears to be very little understood, though the matter is being carefully investigated by more than one experimenter, and some results have recently been published. It seems, however, hardly credible that the maximum pressure to which a structure may be exposed is almost as great a matter of uncertainty; yet such is the case. The papers on wind pressure, above referred to, in spite of the existence of so many anemometers, endeavor to ascertain from a variety of sources, such as previous accidents, and reports of the effect of wind in storms, what the probable maximum pressure has been, both, however, assuming values for purposes of calculation far less than are actually reported. In the same manner, the Commission decided upon a limiting value only a little more than 62 per cent. of a pressure recorded by an anemometer, and believed by them to have actually taken effect in this country.

The fact is, that the motion of the air is, beyond all expression, most complicated. Were it not for this, there would be no necessity for obtaining both the velocity and pressure of the wind, for there is, by a first principle of dynamics, a fixed relation between these two elements; and if one were known, the other could be, at any rate, approximately deduced. In reality, any attempt to treat the wind as having steady motion for more than a very small distance in space, is certain to involve serious error, and the complications which are introduced, from even slight disturbing causes, seem quite beyond the powers of investigation. The engineer is concerned both with prejudicial effect of the wind upon structures, and its useful effect upon wind-motors. In both these cases the conditions are such as to greatly interfere with the steady motion of the wind, and the effect due to locality must be estimated and allowed for. The meteorologist needs observations of the wind at all elevations, and as pointed out by Mr. Laughton in his address, particularly at higher ones, where, judging from the experience of aëronauts, the motion of the wind is nearly as complex as below. Until the motion of the wind is better understood, weather forecasts must be more or less unreliable, and what has been said with reference to the mechanical excellence of the present anemometers and the regular tabulation of results, must not lead to the idea that there is no room for improvement. On the contrary, there is yet much to be done in directions which can here be only briefly indicated.

First, there is great necessity for improvement in the lubrication of the instruments, especially of that portion recording direction, so that in viewing a weather chart of the *Times* it may be certain that in light winds the arrows *really* show the direction and not directly the opposite one. Such an error as this, perhaps from some distant station, causes whole columns of the bulky hourly records to be worse than useless.

Secondly, the reductions for the relative velocity of the wind and cups, if made at all, ought not to be made, as is at present the case, by a factor now well known as the result of much costly investigation, to be erroneous.

Lastly, the locality of anemometers

should be more carefully selected, or at least taken more closely into account, in discussing the effect of wind in storms.

The importance of some reform in the matter of wind measurement is obvious, since it is only by continued observations, under improved conditions, that a more reliable and satisfactory knowledge can be obtained of the aërial ocean in which we live.

METHODS OF IMPROVING RIVERS HAVING A CONSIDERABLE FALL, AND WITH BEDS LIABLE TO SCOUR.

From "Les Annales des Travaux Publics," for Abstracts of the Institution of Civil Engineers.

Rivers with a considerable fall, and flowing in a channel scooped out of a very thick bed of gravel, resemble torrents. When the water is high the fall is fairly regular; but when the water is very low, a series of rapids occur at the shoals, separated by nearly level reaches in which the channel is deep. The removal of one or more of the shoals by dredging only leads to an increase of fall at the rapids above, and is therefore not a satisfactory remedy. Another method of regulating the fall in such rivers is to restrict the channel within low parallel embankments. Such a plan, however, whilst concentrating, and therefore deepening the stream, increases its velocity, and a scouring of the bed consequently takes place till a fresh series of shoals and pools are formed, restoring the river to its original condition. Two methods of improvement have been proposed for this class of river, namely, (1) the restriction of the channel by low training banks; and (2) the erection of movable weirs, accompanied by a partial contraction of the channel. The first method has been carried out on the Rhone for the last twenty years, and the last still remains to be tried.

1. *Improvement by low training banks.*—The method adopted in the first instance on the Rhone consisted in restricting the channel at shallow places by lengths of longitudinal embankments, giving it such a width that, with the maximum discharge and the mean fall, the depth should be 5¼ feet. It is not surprising that this plan did not effect the desired result on such an irregular river as the Rhone, whose depth varies from 2 feet to 26¼ feet, whose width is from 430 to 1,640 feet, whose fall is sometimes only 2¼ inches per mile, and sometimes reaches 31 feet per mile, and whose bed is much scoured by floods. The next plan tried was training the river by embankments, following the natural windings of the river, and placed 590 feet apart. Then, as the river tended to form deep channels close to the concave banks, and left shoals on crossing from one concave bank to the next, the banks were brought closer together at these points of inflection, so as to increase the scour at these points. Though, however, the shallow places were thus improved, the water-level was lowered above, and gravel accumulated below. The defects of the channel are, accordingly, not removed, but their positions are shifted.

In order to regain, in the deep portions of the river, the fall lost by the contraction of the shallow channels, it is proposed to erect compensating dykes, cutting off the deep parts of the channels at the concave banks, and thus to force the river to scour out the shallower parts and obtain a fall sufficient to compensate for the lowering of level produced at other places. This plan would doubtless answer if the bed was sufficiently stable. It is probable, however, that, with a bed so liable to scour, the new channel would become as deep as the old one, and the increased fall would be lost. A system of continuous embankments would set the whole river bed in motion, and the masses of gravel brought down might break the banks and form shoals. Low embankments, moreover, are dangerous for navigation, as they create currents, and vessels may be injured by grounding on them.

2. *Improvement by means of movable weirs, and partial contractions of the channel.*—The contraction of the channel by embankments improves that portion of the channel, but lowers the water-

level. This lowering may be prevented by the erection of a movable weir, lower down the river, which keeps up the level and thus maintains the depth of the channel above. An illustration is given of the movable weir which M. Pasqueau has proposed putting up across the Rhone, at Grigny. It has been designed in accordance with the principles laid down by M. Tavernier, namely, that in rivers bringing down large quantities of gravel, like the Rhone, the wier should be worked from a high fixed bridge above flood level, and that the movable parts should be capable of being raised out of the river. The movable weir, when opened, would not impede the flood discharge; and a portion of the river would have a sill 3¼ feet below low-water level, so as to afford an outlet for the gravel traveling down the river.

THE GREAT STRUCTURES ERECTED IN ITALY DURING THE LAST TWENTY YEARS.

By C. CLERICETTI.

From "Conferenze sulla Esposizione Nazionale del 1881," for Abstracts of the Institution of Civil Engineers.

THE author chooses the bridges of iron and stone erected during the last twenty years as the structures which best exhibit the progress of engineering science, and he compares these modern bridges with those built by the Romans. The characteristics of these latter are grandeur, massiveness, and durability; of the former, lightness, economy, and rapidity of construction.

The Po between Pavia and the sea was never bridged by the Romans, but during the last twenty years four bridges have been built over it. The lengths of these bridges are 577,762,427, and 400 meters, 1,900, 2,600, 1,399, and 1,312 feet respectively, the spans varying from 213 to 250 feet. They are all girder bridges, supported on piers founded at depths of from 60 to 70 feet below highest flood level, and formed of iron cylinders sunk by hydraulic process.

To show the difference between the ancient and modern systems of construction the author compares the Roman bridge across the Danube, one of the boldest of their works, with the modern structures on the Po. The former—1,207 meters (3,960 feet) in length—had twenty-one wooden arches of 50 meters (164 feet) span; and the piers—founded on a masonry platform extending right across the river bed—had a thickness of 17.7 meters; while the piers of the latter, though 28 metres high from the foundation, are less than 3 meters thick at the top. The ancient piers had six times the thickness required for a modern girder bridge, and three times what would now be allowed for masonry arches of 50 metres span. The same immense piers were built throughout the middle ages; the old bridge at Verona, for instance, with two arches of 28.54 meters and 48.70 meters (93½ and 160 feet), has a pier 12 meters thick, though only 3.50 meters high.

The author proceeds to point out the superiority of the modern system of long spans and narrow piers, in leaving the channel free for navigation and the discharge of floods, and avoiding the scouring action caused by obstacles to the natural flow. In some cases old bridges have so impeded the flow as to cause serious inundations above bridge.

The ironwork of the great bridges over the Po was imported from abroad, but the Italians are now constructing their own, some spans of 75 meters (246 feet) having been already built, and others of larger dimensions, up to 100 meters, will shortly be commenced.

The author states that, with few exceptions, only one type of bridge—the lattice-girder—is constructed in Italy, and regrets that little encouragement is given to improvements in design. He mentions a few arched bridges, among them being that over the Celina torrent, which he considers one of the best examples.

The author proceeds to discuss the subject of the incalculable strains to which bridges are liable; from the points of support not being knife edges, as theory sup-

poses; from the variations in cross sections; from the vibration caused by passing trains, &c. Airy attempted to ascertain the strain in a bar of iron from its musical note, but the result was not satisfactory. Better results are obtained by instruments for measuring the contraction and elongation of bars during strains, such as the apparatus of Dupuit and Manet in France, and Castigliano's multiple micrometer, which the author describes.

The experiments made with Dupuit's apparatus upon all kinds of girders show that the actual maximum strains are in general less than the calculated, particularly in arches and in the horizontal members of straight girders.

Iron bridges are also exposed to danger from corrosion, but the author states that Mallet's experiments proved that an iron bar 6 millimeters (0.238 inch) in thickness would not be destroyed in less than 700 years.

The author then gives particulars of some of the principal brick and stone bridges recently erected. Comparing modern with ancient structures, he points out that the former are built with one-third less material than the latter. In ancient structures the ratio between the thickness of the piers and the span varied from one-fourth to one-half, while in modern it has been reduced to one-sixth, and even one-seventh. The average ratio between the thickness of the arch at the crown and the span was 0.086, while in modern bridges it is from 0.040 to 0.031.

The two principal arched bridges erected in Italy during the last few years are the Ponte Annibale and the Ponte del Diavolo. Each of them has a span of 55 meters (180 feet), and thickness at the crown of 2 meters, the versed sine of the former being 14 meters, of the latter 13.55 meters. Circular openings 9.25 meters in diameter, are introduced to lighten the haunches. These are the largest masonry arches in the world, with the exception of one at Chester of 61 meters span, and one on the Washington Aqueduct in America of 67 meters. In the year 1370, however, an arch of 72.25 meters (237 feet) span, and 20.70 meters rise, was erected over the Adda, at the Castle of Trezzo. This arch was considered the eighth wonder of the world, both for size and for the short space of time—seven years and three months—occupied in its construction. The Ponte Annibale and the Ponte del Diavolo were built in twelve and ten months respectively.

Among recent improvements in detail the author mentions the use of hydraulic lime and cement, which allows the centers to be struck very shortly after the completion of the arch; and the use of sand-boxes instead of wedges for slacking the centers, a system which he strongly recommends.

The two above-named bridges were built almost entirely of brick, great economy being thereby effected as compared with stone. The Chester bridge, of 61 meters span, cost £83 per square meter of roadway; the Ponte Mosca at Turin, of 45 meters span, cost £105 per square meter of roadway; whereas the Ponte del Diavolo cost only £34, and the Ponte Annibale £24.

The author concludes by predicting that the limiting span of brick and stone arches has not yet been reached, and anticipates the erection of spans of 100 meters.

Perhaps the strict enforcement of the new plumbing law will be a good thing for householders and plumbers. At least, it should promote somewhat the conditions of better health for the former and better pay for the latter. It only seems reasonable, however, that kitchen sinks, wash-tubs, bath-tubs, hand-basins and water-closets should be constructed in an appropriately ventilated and disinfected tower outside the main residence altogether, but with convenient and comfortable access to such tower's conveniences. In spite of all that metallurgists have done and the most expert sanitary scientists have devised, any but the most remote connection with the ordinary main sewers of cities means more or less frequent deaths in a family, not to speak of protracted, obscure and annoying cases of illness, which do not prove directly fatal. The sanitary arrangements of the great "flat" system of buildings now so popular deserve fully as much attention as the provisions they require for the escape of residents in case of fire.

CANDLE POWER OF THE ELECTRIC LIGHT.

By PAGET HIGGS, LL.D.

From Proceedings of the Institution of Civil Engineers.

II.

Mr. W. Sugg wished to offer a few observations upon a different point to that referred to by Mr. Jones. The author appeared to have taken the cost of gas in New York, when he might just as well have taken the cost of gas in England. The cost, however, was a matter which must be worked out in practice, and if it was found that the cost of the electric light would be very much greater than that of gas, it probably would not be so much employed as gas. That, however, might be left to the future. The part of the paper with which he wished to deal was the first point, namely, the standard sperm candle. The author asked what was a sperm candle, and he had pointed out that the light of a sperm candle was that which would be given from the candle 1 foot all round the light. That was a very good way of expressing a sperm candle, because it was practically what could be got out of it for use, for reading or for work; but unfortunately it was not the standard looked upon by Parliament as being the standard sperm candle. The light of a standard sperm candle was the light given from a point in the center of a candle, and the calculations with the photometer were made upon that assumption, that the point of light in the center of the candle was the whole of the light of the candle. He had found it practically an extremely difficult thing, with such an arrangement as that, to carry out experiments and calculations with regard to lighting various areas, because with that theory to deal with, viz., the central point in the candle being the whole of the light, it was evidently a difficulty, when it had to be worked out for estimating the degree of illumination of areas. The plan which the author proposed, of taking 1 foot round the candle for that purpose was a good one, and could be usefully adopted for many purposes. As he had pointed out before, the standard sperm candle was an india-rubber rule, and it seemed strange that for so many years it had continued to be used as a standard when so fallacious, and which was known to be fallacious so far back as 1868, through a series of experiments made by Mr. T. N. Kirkham, M. Inst. C.E., then the engineer of the Imperial Gas Company, in which he showed how very different one candle was from another. Those experiments had lately been repeated, and he supposed from time to time they would be repeated again; but what would be the result of these repetitions he could not say. The Standards of Light Commission appointed by the Government had also endorsed the opinion, given by Mr. Kirkman and himself, that the standard of light adopted for England was a bad one. There were other standards of light which were really standards of light, and were not such as that derived from degrees of temperature, as the author of the paper seemed to desire. He did not himself see what the temperature of the flame would have to do with its illuminating power, except, as Mr. Crompton had stated, with regard to the incandescent lamps. Incandescent lamps of course would give a very much higher illuminating power as the temperature was raised; and therefore in that respect, supposing one incandescent lamp were measured against another it might be useful, but as comparing the illuminating power of electricity with that of gas, or any other standard, it seemed to him that it was a bad thing, and would result in erroneous statements. When there were found differences in the illuminating power of 16-candle gas of from $1\frac{1}{2}$ to 2 candles with the best candles obtainable, it would be seen that when that was magnified up to the high illuminating power of the electric light, errors would arise which were surprising. With respect to the tables adopted by the author, he had introduced, as Mr. Crompton had observed, "heat-grammes," and sundry other terms unintelligible to those who did not follow very closely the line in which he had been working; but Mr.

Sugg could point out that there were several standards at the present moment better adapted for the purpose of testing the electric than the standard candle. There was first of all the gas standard introduced by Mr. Vernon-Harcourt, one which could be carried out for the purpose of estimating the standard candle accurately at any time and under any circumstances. The method that he adopted, taking a certain quantity of pentane, a product of petroleum, distilled in a certain manner, mixing a certain quantity of it with air and burning it in a proper apparatus, appeared to give a perfect idea of what a standard candle should be. That was the only one, he believed, in which the value of the light was an exact standard candle; but there were others, for example, that of Mr. Keates, in which he used spermaceti oil, and burnt it in a lamp, producing a light of 16 candles, and that light was much more easily used for the purpose of testing the electric light. He had used it himself for that purpose, and found it going for weeks without variation, so that he believed it to be a much more reliable standard than the sperm candle. The next one after that was a standard of two candles made by Mr. Methven, assistant engineer of the London Gas Company, in which he used the ordinary common gas supplied for lighting; and if there was as he said no variation in that standard when used with common gas, and Mr. Sugg believed there was a great deal of truth in what he said, it would be certainly better than the candles, and that notwithstanding there might be slight variations in it; this standard of his would be found much more suitable to the electric light. The next was a 10-candle gas standard of his own, and there were several others which were very useful; and if the electric light was to be estimated for its illuminating power, it would be better to estimate it by such a standard as these than by the fallacious standard adopted of a parliamentary sperm candle. There was one remark made by Mr. Crompton on which he would make an observation, and that was as to the manner in which testing the electric light for illuminating power could be carried out. In the case of gas, the assumption was that the light was given in a circle all round the burner—equal in all directions—and nearly all round in a vertical circle. It was not so with the electric light. With the electric light the light came from between the two carbons, and the strongest light was in one direction; it did not light equally raund the vertical circle, neither did it light equally in the horizontal circle; because on whichever side of the center the carbon rested, one side or the other, a greater light was shown. It could be seen with a Bunsen photometer that this variation would produce very great errors. With regard to the incandescent light, that, of course, could be tested in exactly the same manner as gas, except that it raust be tested as a flat flame burner; because he presumed that the light was given more strongly in the direction of the one side of the loop than it was across the loop, so that if the mean of the edge and flat of the lamp was taken a very good result would be obtained. But with the arc-light it certainly did seem necessary that a correction should be made when it was tested with a photometer horizontally or at an angle, for an evident error existed in the value of the result, caused by the fact of the light not giving its light in all directions alike, as supposed by the construction of the photometer. With the Jablochkoff light the result more nearly approached that given by a candle than in any other, with the exception of the Jamin, which was the reverse of the Jablochkoff. Either of those could be easily tested in the manner he had stated; but with the arc-lights it would be necessary to make the correction, and he had not seen that that correction had ever been made.

Mr. J. N. Shoolbred said, he wished to refer to the tables contained in the paper. There was a very material difference in the way they were arrived at, which the author seemed hardly to be aware of, and which ought to be pointed out. All the lights named in the first table were lights that had been produced and measured directly from the electric machine, or the dynamo itself. The second table, on the other hand, represented the result of experiments carried out by Sir William Thomson upon a single Swan light, at which Mr. Shoolbred was allowed to be present, and in which the Faure accumulator battery was used, the current being taken direct from that instead

of from the dynamo. The results showed points of considerable interest, and, he thought, opened a very large future for incandescent-lighting where a steady current was used. Sir William Thomson not being fully satisfied with the photometric measurements, and having to leave town, allowed him to make some further experiments, and the result of the second series of experiments shown in the tables and curves annexed. The series of experiments was carried out upon a single Swan light and a single Maxim light; increments of current being made by successive additions of five Faure cells at a time. The photometric measurements in the second case were carried out with the instrument to which Mr. Sugg had referred, and with Mr. Keates' 16-candle sperm-oil lamp as the standard of reference. The oil consumed was accurately weighed, and there was every reason to believe that the measurements were carried out accurately. The curves represented severally the candle-power, the measured potential, the intensity of the current, and the amount of mechanical energy in HP. This last was the simplest manner of putting the mechanical energy expended; he quite agreed with Mr. Crompton, that the author had needlessly complicated the paper by introducing gramme-degrees, foot-lbs., or heat-units; all of which could be deduced from the ratio generally made use of—that of candle-light per HP. The amount of mechanical energy converted into electrical energy was indeed the basis of the whole of this mode of generating electricity. In practice the condition of incandescent lights, when working direct off a dynamo-machine, and without an accumulator, was represented approximately by the diagrams (see following page). Such being the limit under the ordinary conditions, the value of the intervention of the accumulator was represented by the gradual progress towards the right. It would be seen how greatly the intensity of the light could be increased, and at the same time its economic value raised, in proportion to the current expended, by using the steady current of a storage accumulator. In another way the economy of these lights could be augmented; inasmuch as their life would be considerably lengthened owing to the use of the steady current. It had been mentioned, that if the incandescent-lights were urged beyond 16 candles there would be a gradual deposit of carbon on the glass, and the filament of carbon would be destroyed. He had noticed himself the phenomena referred to of the deposit of carbon, but that was owing to the improper use of the lamp; for if a lamp which was only intended for 16 candles was pushed to 25 or 30 candles, there would of course be produced an extra strain. But to say that

TABLE OF COMPARATIVE EXPERIMENTS WITH FAURE ACCUMULATOR ON INCANDESCENT ELECTRIC LIGHTS.

1. SWAN INCANDESCENT LAMP IN CIRCUIT.

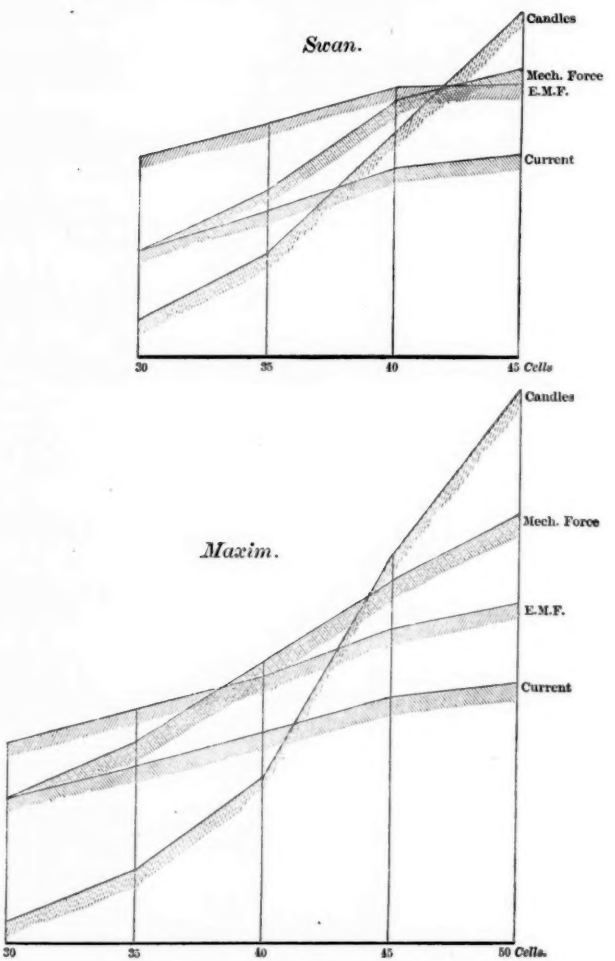
Number of Faure Cells used.	E.M.F.	Current.	Light.		Mechanical Energy.*		
	Volts.	Ampères.	Standard Candles.	Becs Carcel.	HP.	Kilo-grameters.	Heat Units (Joule).
30	73	1.28	22.4	2.36	0.125	9.52	5.3
35	85	1 84	65.6	6.91	0.209	15.94	8.9
40	97	2.38	141.0	14.84	0.309	23.53	13.2
45	104	2.50	204.0	21 47	0.348	26.50	16 3

1. MAXIM INCANDESCENT LAMP IN CIRCUIT.

30	74	1.81	16.0	1.68	0.179	13.65	7.6
35	85	2.24	45.3	4.77	0.255	19.41	10.9
40	98	2.59	101.1	10.64	0.340	25.87	14.5
45	113	3.00	229 0	24.11	0.454	34.56	19.4
50	124	3.20	333.0	35.05	0.531	40.45	22.6

* The mechanical energy lost in charging the accumulator from the dynamo is not included.

Comparative Experiments in Incandescent Electric Lighting with Faure Accumulator.



incandescent-lights were limited by all the makers to 16 candles was totally fallacious; because they could be made of whatever candle-power was required. Just the same as a gas-burner could be made to comsume 2, 3, 4, or 5 cubic feet of gas, so the resistance of the incandescent-lamp could be altered so that it would give from 10 to 40 candles or more. With regard to the proportion of candle-light given off per HP. absorbed with the incandescent-lighting, Mr. Swan had himself some two years ago limited it to from 150 to 200 candles at the outside per HP. There appeared to be a great deal of difference with regard to the cause of the large discrepancy between the proportion of light produced per HP. absorbed, with the arc over the incandescent system; an explanation given some time ago by Mr. C. F. Varley seemed to point to the true cause. It was suggested that a much larger proportion of the current was used in warming up the carbon to incandescence than was required to pass from that stage to the production of the arc; and in this greater light-giving value of this last portion of the current might be found some explanation of the apparent discrepancy. If what was indicated by the diagrams about the use of an accumulator in conjunction with the dynamo was correct (practically the substitution of the 40-cells vertical line, in the diagrams, instead of the 30-cells one), it might be argued that incandescent-lights might, by its use, be very much more economical in their results than they had hitherto been. The fact that more duty could be got out of gas when used in a gas-engine than when used for illuminating purposes was not surprising. In the report of the Committee of the House of Commons, in 1879, On Lighting by Electricity, it was pointed out that the heat-giving properties of gas exceeded considerably the light-giving ones. Bunsen had shown that the light-giving properties were only 6½ per cent. in 100 volumes, whereas the heat-giving properties were no less than 87 per cent.

Professor TYNDALL remarked that he had not dealt much practically with this question of determining the candle-power of the electric light. He had, in association with Mr. Douglass, done something of the kind, but that was a long time ago. He noticed, of course, that contending parties were here upon the platform, but from those he begged to entirely abstract himself. With regard to Table I., he believed the results might have been predicted *à priori*. It must be remembered that the so-called electric light was a thing of an exceedingly composite character. It had an outflow of rays that were entirely incompetent, even when they impinged upon the retina, to excite vision. Years before the present amazing powers of the electric light were developed, he had experimented upon the light produced by a battery of fifty Grove cells, which evoked what in those days might be called a very powerful electric light, and found the invisible radiation, meaning by that the radiation which was incompetent to excite vision, to be 90 per cent. of the whole. He was afraid it was impossible to get rid of this condition. This invisible radiation appeared to be, so to say, the substratum of the visible. The luminous rays must be built, as it were, upon the non-luminous rays. The same was the case with the sun itself, as Herschell was the first to prove. Müller found that the luminous rays of the sun were only one-third of the total emission by the sun; that the invisible, obscure, calorific rays emitted by the sun were two-thirds of the total radiation. In the case of the electric light the invisible rays were by one series of experiments proved by himself to be 7.7 times the visible; and in another series of experiments, made according to a totally different method, the invisible calorific rays proved to be 8 times the visible. With regard to the sun, as he had said, its invisible radiation was twice as great as its visible radiation; but higher in the atmosphere, above the screen of aqueous vapor that overspread the earth, if a spectrum of the sun was obtained at a great elevation, it would be found that then the obscure radiation of the sun approximated to that of the electric light. He received a letter some time since from a gentleman who had been experimenting at a height of 12,000 feet above the sea, in a very dry region of the earth in the Sierra Nevada Mountains in California, and he declared that there was an enormous extension of the invisible spectrum of the sun in those regions. Probably at the limit of the atmosphere the invisible radiation of the sun, would represent

six times the energy of the visible radiation. With regard to the table, the author of the paper took into account the total amount of power absorbed, and the question was, how much of that power was converted into luminous rays, into those rays that were effectual for vision, and how much into rays that were not effectual for vision. On theoretical grounds he should have inferred that the table must be as the author had stated, and that, as Mr. Crompton had remarked, the more intense the power was made by the introduction of a resisting interval between the two carbons of the arc, and the higher the electro-motive force invoked to urge the electric current across the interval, the greater was the proportion of the luminous rays introduced into the total radiation. In the first lamps mentioned, the foot-lbs. per candle-power was very small compared with the smaller lights. This simply expressed, in the case of the Werdermann and in the case of the incandescent-light, that in the intense arc-lights a greater fractional part of the total energy was converted into wave-motion competent to excite vision, than when less power was used.

Mr. W. Atkinson did not know whether the author had stated at what distance the experiments had been made with the light. He understood that there was great difficulty in arriving at any conclusion as to the power of the electric light, dependent upon the different distances at which the experiments had been tried. He believed it had been discovered that, even in comparatively very short distances, in a room within the space of a few feet, very varying results would be obtained. The rays of the electric light were probably readily absorbed by the atmosphere when humid, as Dr. Tyndall had mentioned, in the case of the sun. Then with regard to the economy or the cost of gas, the element of the destruction of fittings in a house had not been referred to. If the electric light and the gas light were compared, it was clear that, to a consumer, the electric light would be more economical, because there would be no destruction and no dirt.

Mr. J. N. Douglass said his experience with the electric light had been entirely with arc-lamps. It was a pity that the comparison in the paper as to the cost of gas and of the electric light, had not been made with gas in London instead of in New York, because the cost of gas in New York was about $\$2\frac{1}{2}$ per 1,000 cubic feet, while in London it was about 3*s*. Following the figures of the author, he found he had given a 4-light chandelier of 64 candle-power, as costing per hour in New York 20 cents for gas, the burners being of the efficiency of the London standard burner. A burner would give about 5 candles per cubic foot of gas consumed; therefore, the above result would be got in London at the cost of about 1.8 per hour, as against 8*d*. per hour in New York. As Mr. Jones had pointed out, the only cost given in the Paper for the electric light was that of the motive power, and that was stated to be 4.1 cents, being more than twice the cost of the gas light in London. There appeared here the same difficulty of comparison that was met with in lighthouse illumination; and, from his experience, he might say that if the electric light could be fairly compared with oil or gas as consumed in lighthouses, it would be found that, with the arc-light about ten times the amount of light per unit of cost was obtained with electricity above that of gas. Unfortunately, the element of cost of plant and of additional cost of labor came into play; and up to a certain intensity, at a lighthouse, oil was the cheapest light. But then an intensity could be attained ten times that of the oil with the electric light; and there would be about five times the amount of light per unit of annual cost than with the oil; however, the first cost and annual maintenance were doubled. If that first cost could only be reduced to that of the oil or the gas-light, electricity would, no doubt, prevail. With regard to the measurement of candle-power, there appeared to be difficulties with the electrical mode of measurement; and he, for one, would not be disposed to accept any apparatus for electric light, unless he measured the light photometrically in addition to the system proposed by the Author of the Paper, because, as pointed out by Mr. Crompton, there was the quality of the carbon coming into play, which was liable to considerable variation. It was quite possible in the same bundle of carbons to get differences

of quality of certainly 50 per cent. With regard to the candle-power, he saw no difficulty in using an ordinary candle with care. Any one who was used to it could arrive at results, certainly within 5 per cent.; and to measure the electric light, which varied within an hour 50 per cent., surely the candle was near enough as a unit of comparison. The great difficulty with the candle was the difference in color. That was, however, easily got over, if it were wished to reduce the actual candle-measurement to the intensity at the actual candle-flame color by coloring the electric light with yellow glass, and bringing it to the color of the candle.

Sir William Armstrong, President, said he had had considerable experience with the incandescent system of lighting. He had used it in his house in the country for nearly a year. He had gone through many troubles and difficulties, such as early experimenters always had to encounter; but, upon the whole, he could decidedly say that his experience had been satisfactory. No doubt the comparison of candle-power between the different systems of lighting was a very important matter; but it was by no means the only consideration that presented itself. He commenced with attempts to make the arc-light available for domestic use; and after trying various systems and various arrangements, he came to the conclusion that no possible improvement that could reasonably be hoped for would make it suitable or desirable for domestic purposes. He then tried Mr. Swan's system, and with the lamps which he furnished in the first instance, and which were made very carefully, no doubt, by hand, the endurance and illuminating power were exceedingly satisfactory. He came to the conclusion that each single lamp gave about as much light as an ordinary duplex kerosene lamp, usually estimated al about twenty-five candles. When the company commenced their operations, they changed the system, and instead of using single lamps, they used two lamps in series—at least they recommended the employment of two in series, instead of one in parrallel. Owing, perhaps, to imperfect experience, he found the durability of the new lamps much less than that of the first lamps supplied; but after a good deal of disappointment and change, he eventually obtained lamps which, in pairs, fairly represent what the single lamps did before. It was premature to attempt any comparisons either as to the illuminating power or cost of renewals, for it was quite clear that the whole system was yet in its infancy. He could state from his own experience that there was the widest possible difference between the lamps supplied. He had some lamps at the present time which had been in use from the very first, whereas he had also had some that failed after a few hours' use; therefore, until the manufacture settled down to something mature, and the difficulties of starting were fairly got over, there could hardly be a judgment as to the capabilities of the lamps, either with reference to endurance or illuminating power. This much he could say, that no deficiency of candle-power or endurance such as had been attributed to them would induce him to abandon the system. Gas was an admirable means of lighting in its proper place, but in private rooms it was undoubtedly very objectionable. The incandescent light had no connection whatever with the atmosphere, and therefore had no contaminating effect upon it; it had very little heating effect; it was perfect in color, perfect in steadiness, and in fact was the perfection of lighting for domestic purposes. That, at least, was his experience; and he had no doubt that difficulties which had arisen, and were arising, would be got over, and that the incandescent lamp would attain to a perfectly satisfactory state. The number of lights in his house was sixty, that was thirty pairs. He had more, but could work that number at the same time. The source of power was a turbine situated nearly a mile off; and with 7 HP. he was enabled to maintain those sixty lights. Most of the lamps that had failed had not failed through the actual wearing out of the carbons so much as from defects in their manufacture, from points at which there seemed to be some defect which made them liable to give way in use. He felt, however, sure that, when the manufacture of the carbons were perfected, all difficulties of that kind would be got over. The light in his case, using water power, was highly

economical. There was the cost of the laborer's attendance upon the machine at night to supply the sixty lights, and the only other expense was the cost of renewals, which would certainly not be very serious, according to his past experience. One point he might mention was the extreme importance of having an absolute uniformity of motion in the generator. The smallest variation immediately produced a disagreable twinkle upon the lights; and so sensitive were they, that while he used belts made in the ordinary way with joints, he could count the revolutions of the wheel; that was to say, every time a joint ran over the pulley it made a sufficient variation to cause a slight effect on the light. He could not obtain an absolute uniformity untill he used an endless belt made life a flat chain of leather links stamped out of the sheet, and joined together by putting a pin through, a form of belt now pretty generally used in cases where very even and regular motion was required. He was afraid that unless the gas-engine was supplemented by means to obtain a very steady and uniform motion, the absolute steadiness of light which he had attained would hardly be obtained from it; but no doubt there were means, when attention was directed to the attainment of that particular object, which would be found to remedy any inequality.

Dr. Higgs remarked in reply, through the secretary, that the results glven in the paper were intended partly to be intercomparative, and partly were an endeavor to reduce the observations of different authorities to a common standard. This common standard he assumed to be represented by the "energy" absorbed in the light-center. He did not suppose that "temperature" and light were related; but that if light were any form of energy, then that light would be related to the energy of the light-center; he had measured this energy in heat-units and not in "foot-lbs. per candle-power," as suggested, because he did not know what a candle-power was, in which ignorance it seemed he did not, judging from the remarks of those who had favored him with their criticism, stand alone, and he thought he did not know what a heat-unit was. Between the energy as measured in heat-units, not the temperature, he had found the relation to be as stated. But besides criticism, he had thought to elicit facts and measurements from others.

CORRESPONDENCE.

Mr. K. W. Hedges observed that the author, in referring to the way in which he measured the electric light in comparison with the method adopted by Sir W. Thomson and Mr. Bottomley, did not mention what that method was. The great difficulty with powerful electric lights seemed to be the variation of their color as compared with the present standards, the sperm candle and carcel burner. He thought that, failing a better standard, the difficulty might be obviated by photography, either as adopted by Captain Abney by photographing the spectrum, or in a simpler manner by photographing the luminous crater in the positive carbon. The intensity of the light was greatest at the latter point, and by interposing glass of known opacity between the light and the sensitive plate, and noting the time taken to produce a photographic image, the comparative amount of light from any two sources might be ascertained. He noticed the author's opinion that incandescent lighting was theoretically six times the cost of arc lighting. This would make the incandescent light as dear or dearer than gas, and might deter the introduction of the electric light into theaters and crowded rooms where it was much needed. The cost of arc-lighting was considerably less than that of gas, the only drawback being the color which did not harmonize with gas. He thought the difficulty might be got over by enclosing the arc lights in colored globes so as to tone the light to the color of gas. From an experiment in one of the picture galleries at the South Kensington Museum, he found the loss of light to be less with a suitably colored globe than with an opal one. If two or more arc lights were enclosed in a lantern, the fluctuation of any one would be less noticeable, and one could be turned out if necessary. With a margin of six to one in favor of such a light which could not be at once detected by the unitiated as that from an electric source, and which had all the advantages pos-

sessed by incandescent lights, the saving in cost would alone cause the arc light to be preferred to the latter.

Mr. RADCLIFFE WARD observed that if the subject had been the cost of the electric light as against gas, he had no doubt that several engineers would have been prepared to prove that, even on the very restricted scale of electric-light installations now existent, gas could be competed with. He would first direct attention to the passage in the paper, wherein it was stated that a Serrin Lamp and Gramme Machine gave a light of 3,600 candle-power, when the arc resistance was about 1¼ ohm.; and that with about the same arc resistance a Crompton lamp yielded a light of 3,600 candle-power; also in the case of the Gramme and Serrin the weber or amperè current was stated to be 45.7; in the case of the Crompton lamp only 24. This, according to his experience with good carbons, was what frequently gave 3,600 candles as "diffused beam;" such an extraordinary difference between the Crompton and Serrin Lamp as 24 to 45.7 required some explanation. To put such figures in the paper, without comment, was misleading and puzzling to any one not a practical electrical engineer. Why was it not stated what were the machines used, that was, what type? He should be particularly interested to know what class of Gramme machine was used. The author did not appear to think much of the carbon element, whereas Mr. Ward agreed entirely with Mr. Crompton, that the carbon question was most influential and important, not only in arc but also in incandescent lighting. According to his experience, and indeed the point was self-evident and could be easily foreseen, one of the most important features in the construction of incandescent lamps was the form of the carbon filament regarded as a structure; the chief point being the proportion of indenting surface to the total mass, and sectional area. He thought, in using the electric light for domestic purposes, it would be advisable to employ a dynamo machine during the day to "charge" accumulators placed in some convenient position in the house, and then work off the accumulators at night direct to the lamps. This method would be preferable on account of being able to work in a house with a lower electromotive force. With respect to the comparative cost of the electric light, he would not go into details; but the cost of lighting Whitehall, as now practised by gas, was considerably more than if it were lighted by an electrical system, such as could advantageously be employed. Finally, he would suggest that the cost of lighting lighthouse lanterns by electricity might, in the not distant future, be much reduced by obtaining the current from a large central electric generating station in the neighborhood; say the South Foreland lights from "Dover Town electric generating station" that was to be.

THE COST OF ELECTRIC LIGHTING BY INCANDESCENCE.

By WILLIAM CROOKES, F.R.S., &c.

"London Times."

FOR more than six months I have had the principal reception rooms in this house almost exclusively lighted by incandescent electric lamps, the electricity being generated on the premises; and as so many different opinions have been given as to the expense of lighting by incandescent lamps, some saying that electricity is many times more expensive than gas, while others maintain that it is cheaper than gas, the results of my own private experience in electric lighting may not be without interest.

The dynamo machine—a small Burgen—is driven by a 3½-horse power Otto gas engine, which under favorable circumstances will develop 5-horse power. Owing to the absolute necessity which exists in a private house in this neighborhood that there should be no smell of unconsumed gases and no noise of machinery either in the house or out in the street to annoy my neighbors, it became necessary to add silencing chambers to the air inlet and the exhaust pipe, and to carry the products of com-

bustion high up on to the roof. The obstructions thus put in the way of the free working of the engine necessarily affect the horse power, so that when a further deduction is made for the power absorbed in running the machinery when no electricity is being generated, I find I have not more than two horse power available for the production of electricity. This is far from sufficient to drive the dynamo machine to its full power, therefore I lose greatly in efficiency both in the engine and the dynamo machine. However, I have only to deal with the facts as they show themselves in my experience. The total necessary expense of the installation has not exceeded £300, including wiring the house and making the lamps, although the actual expense to me has been much more, as I had to excavate and build underground rooms for the machinery. Where stables or outbuildings are available, or if a little noise is not prohibited, a less expense will give more available electricity, and where steam power can be used the cost will be diminished fourfold. The gas engine requires five minutes' attention every day to fill the oil cups and start it. Once started, it will go on without attention for six or eight hours. It is overhauled and cleaned once a week; an engineer does this on a Saturday afternoon, at a cost of 2s. 6d.

The maximum electric current which I can get is 11.5 ampères through an external resistance of 12 ohms. The lamps fed by the current are distributed as follows:

In the library I have ten 20-candle lamps; in the dining room I have ten 20-candle lamps; in the drawing room I have a cluster of twenty-one 4-candle lamps in an electrolier in the center of the room, and six 20-candle lamps. One or two lamps are in other parts of the house; the total number of lamps about the house being about 50. I cannot, however, have this number alight at once, as the machine as at present driven will not feed so many. It is, however, sufficient to light any two rooms perfectly, and the third partially.

Switches are placed in cupboards in each room, so as to turn any desired combination of lamps off and on. Main keys, cutting off the whole of the current at once, are placed in the engine room, and also in my laboratory at the place whence the main wires diverge to the different rooms

Owing to inexperience in adjusting the strength of the current to the kind of lamp used, and to the variety of systems, &c., I was then testing, the breakages during the first three months were somewhat numerous. For the last three months, however, since passing the experimental stage and settling down to a definite system, I have used lamps made by myself, and during this time only one lamp has gone.

The gas burnt in the engine when the machine is feeding its maximum number of lamps (twenty-two 20 candle lamps) is about 550 cubic feet in five hours, costing at 3s. 2d. per thousand 1s. 9d. Assuming that the light is required on an average five hours a night all the year round, this would come to £2 9s. a month, or £31 17s. per annum.

To obtain, not an equal amount of light, but a fairly good light from gas, to replace this amount of electric light, would take 30 gas burners, each burning 5 feet per hour, or 750 cubic feet in five hours, costing 2s. 4½d., or £3 6s. 6d. per month, or £43 4s. 6d. per annum.

The expenses, therefore, per month stand as folows:

Electricity—

Gas consumed in engine....	£2	9	0
Engineer once a week to clean and oil machinery.....	0	10	0
	2	19	0
Lighting by gas alone..	3	6	6
Balance in favor of electricity per month.....	0	7	6
Or per annum.....	£4	17	6

I have here charged only the current expenses. Strictly speaking, I ought to charge interest and wear and tear, but these are more than counterbalanced by the incidental advantages of electric lighting. With it the ceilings do not get blackened, the curtains are not soiled with soot and smoke, the decorative paint work is not destroyed or the guilding tarnished, the bindings of books are not rotted, the air of the room remains cool and fresh and is not vitiated by the hot fumes from burnt or semi-burnt gas,

while fire-risk is almost annihilated, as no lucifers are used, and the lamps are high up out of reach.

In the above statement I have compared electricity with gas as an illuminating agent. This is giving gas an unfair advantage. The twenty-one electric lamps in my drawing-room do not replace gas jets, but wax candles, whilst the incandescent lamps in the dining-room replace candles and oil lamps. The actual expense of these per night comes to three or four times the cost of electric illumination.

Moreover, I am producing my electricity at an extravagantly dear rate. The dynamo machine works only about half power, and this greatly reduces its efficiency; while Messrs. Crossley tell me that a consumption of over 100 feet of gas per hour ought to give me double the power I get out of the engine; and doubtless it would do so were it not for the back pressure produced by the silencing boxes.

When electricity is laid on to our houses as gas is, all these extra expenses and difficulties will disappear; and if, as I hope I have shown, electricity, heavily handicapped as it is in a private house, compares favorably with gas even in the matter of cost, it will necessarily be far cheaper than gas when it is supplied wholesale from a central station.

THE CONSTANT SUPPLY AND WASTE OF WATER.

By Mr. GEORGE F. DEACON, M. Inst., C.E.

A Paper read before the Society of Arts.

The waste of water is an evil, the author urged, of the highest importance, and one happily that may be prevented at a comparatively insignificant cost. By "waste" he meant not misuse, but loss by leakage between the point where a supply enters the towns and the taps or other domestic fittings. This waste he divided into two kinds, "invisible" being generally underground, and always incapable of detection by superficial examination; and "visible," being generally above ground, and otherwise capable of detection by superficial examination. The loss from invisible waste is, under ordinary circumstances, very rarely detected, unless the amount is so great as to impoverish the supply to neighboring houses beyond the limits of endurance. In the case of visible waste, however, generally caused by defective house-fittings, the conditions are essentially different; sooner or later, the plumber is called in, and repairs of some kind are effected. As compared with the hidden waste, therefore, individual cases of such superficial waste are of a more or less intermittent character. The continuous waste, and the aggregate of intermittent leaks, amount to a certain fraction of the whole supply. Take the case of a £40 householder, with his wife, three children and one servant, six persons in all; if he draws on an average, 15 gallons per day for each person, that is 90 gallons per day in all, he is a very large consumer of water indeed; but if in any part of his premises, above or below ground, there is a leak no larger than the diameter of a moderate-sized sewing needle, discharging water continuously under a pressure of 45 lbs. per square inch, his share of the water supply is at once doubled, and if the needle leak were stopped, two houses instead of one could be supplied. The aggregate sectional area of 1,667 such needles is one square inch. It has been ascertained that such invisible leaks are exceedingly common, and that they vary in size from the sewing-needle, or even less, to the square inch, or even more, in which last case the single leak under the assumed pressure of 45 lbs. per square inch, would supply 2,000 such households, or 6,000 persons. The number of these leaks, although in the aggregate large, is small as compared with the leaks from domestic fittings, causing visible waste, but owing to their much greater average size, and to the much greater pressure under which the water flows through

them, the total waste from these invisible defects often greatly exceeds the total waste from superficial defects.

By three classes of figures and their combinations, we can therefore represent by diagrams all the modes of flow which occur in practice. The first mode, constant in velocity, and long in duration, representing the two classes of waste, invisible and visible, and shown by lower and upper rectangles respectively. The second mode, also constant in velocity, but of comparatively short duration, representing the draught of water through a tap, without a cistern between it and the water main. The third mode, varying in intensity, quickly attaining its maximum, but slowly diminishing, caused by the passage of water through a ball-cock into a cistern. Now, in any ordinary case of water supply, these three modes of flow co-exist, and their resultant from noon on one day to noon on the following day, is distinctly shown from minute to minute, by the position of the upper horizontal line on the diagram. Such a diagram may be automatically reproduced by the motion of the water entering any district through the main supplying that district, and that the facts thus made known lead to important results. Having explained these diagrams, the lecturer proceeded to show each of the methods which have been employed for the detection and prevention of waste.

The first and simplest, but crudest of methods, consists merely in restricting the supply, by turning off the water at the main. Owing to its extensive adoption, there are millions of people in this country to whose houses the water comes only during 20 to 100 minutes a day. This most harmful and most expensive of methods for the restriction of waste, is commonly known as intermittent supply.

Its evils are: 1. Ordinary cisterns for the storage of portable water are dangerous, on account of the great difficulty of keeping them constantly clean, while the mode in which they are commonly connected with water closets, renders them still more dangerous. Under intermittent supply, such cisterns are necessary: under constant supply, they are not necessary. 2. When, under constant supply, the flow is daily intercepted, the water left in the main and pipes gradually finds its way out at taps opened in the lower parts of the district, and at defects in the pipes or fittings. By this means, the main is partly emptied, and an in-draught takes places at defects in the higher parts, to fill the void thus occasioned. This in-draught may be air, or it may be—and frequently is—foul water. The leaks most difficult of detection, and therefore most permanent, are those immediately above sewers and drains; the air thus forced into the main is frequently that of sewers or drains. The water similarly forced in is too commonly that of foul closet-pans, the outlets of which are stopped, or partly stopped. This foul air or water is infused into and served with the next day's supply. 3. The whole of the twenty-four hours' supply for use, misuse, or waste is concentrated in a fraction of the twenty-four hours. If the duration of supply is one hour, the average rate of flow in the mains must be twenty-four times as great as with a constant supply, in which the waste has, by other means, been similarly reduced. The result is that, during that hour, the pressure in the mains is greatly diminished, and the consequence in case of fire is shown in London by the almost universal necessity for the use of fire engines. When a fire takes place during the intermission of supply—that is during twenty-three hours out of the twenty-four—there is no water to be had in the service man until the arrival of the turncocks, and the pressure is then so far diminished by the leakage that fire engines are still necessary.

The second method of restricting waste, like the last, is simple, but expensive and crude. It consists in nothing more than replacing all, or nearly all, the pipes and fittings, both public and private, with new ones of a better kind. The first well-known case of its adoption was by the Norwich Water Company, who obtained, in 1859, the necessary Parliamentary power to apply this method in its broadest sense.

The application of the method was instrumental in reducing the rate of supply during 24 hours from 40 to about 15 gallons per head—which comparatively low consumption was maintained by one house-to-house inspector to about

30,000 persons. Unless it can be shown that defects incapable of repair exist in all the fittings, and that the mains and pipes are, throughout, in such a condition that existing leaks, even if detected, could not be usefully repaired, this method is obviously wasteful alike of the money of the public and of the water authorities. When such work has been performed, the system of distributing mains and fittings is left precisely as it would be in new waterworks, carried out with the same skill and care. But the fittings and pipes do not remain new: they deteriorate rapidly, and if left to themselves, their condition is, in time, little better than before their renewal. Obviously, therefore, absolute renewal, even under the most perfect conditions, is not of itself sufficient.

The third method of restricting waste is simply the system of house-to-house inspection carried out without renewal of the fittings or pipes. But house-to-house inspection is incompetent to discover invisible waste, and for each visible leak detected, the inspector of necessity visits many private premises in which no waste is taking place.

By the fourth method of restricting waste the examinations are confined to the particular premises in which waste is actually taking place, and the hidden as well as the superficial waste is detected. In the year 1865, Liverpool had adopted the first method of restricting waste, viz., the intermittent system, and had combined with it the second method, or house-to-house inspection, on the scale of one inspector to 111,000 persons. This number was gradually increased, until in 1870 it became one inspector to 58,000 persons; in 1871 one inspector to 43,000 persons; and in 1872, one inspector to 36,000 persons. During the same time the first method of restricting waste was applied with increasing stringency, by diminishing from time to time the number of hours supply per day until it was reduced to 9 out of 24; but, notwithstanding these precautions, the rate of supply gradually increased, and the condition became so critical that two or three such dry seasons as sometimes occur in succession would have brought about a disastrous water famine. In this emergency the Liverpool Corporation proposed to seek for Parliamentary powers to enable them, if necessary, to adopt the second method of restricting waste—*i. e.*, the method of renewal. But the townspeople disallowed the expenditure necessary to support the bill in Parliament.

The first method of restricting waste, viz., restricted or intermittent supply, had for many years been applied. The second method, house-to-house inspection, and repair or renewal of detected cases, had long been in operation, with a yearly increasing staff. There was no known method left, and it, therefore, became imperatively necessary to investigate the causes of waste more minutely, and, if possible, to devise some method by which a larger proportion of that waste could be brought to light. An experiment, extending over a population of 31,080 persons, was then made, at very considerable cost, by the Corporation of Liverpool. That experiment was directed to the determination of the exact nature of the waste, and it was proved that the different methods of restricting waste produced the following results respectively:—The population of 31,080 persons, as left by ordinary house-to-house inspection, with one inspector to each 86,000 persons, required a supply of 33.5 gallons per head per day; on the application of intermittent service, by which the supply was limited to 9½ hours out of the 24, the rate of supply became 19.5 gallons; by the detection of waste by baring and examining, and, if necessary, renewing the pipes, and by employing nearly all the Liverpool inspectors in this comparatively small district, the supply, notwithstanding the abandonment of the first method, and the restoration of constant service, was reduced to 13.3 gallons. The results of this costly experiment were ascertained by 14 ordinary positive and intergating meters placed upon the mains supplying different sections of the district. Among other things, it was conclusively shown that complete renewals of either mains or fittings was an unnecessary and wasteful process, and that if only the locality of each leak could be brought to light its prevention could be effected at a comparatively insignificent cost.

The possibility of detecting the existence of a leak, by taking advantage of the conduction of the sound caused by

that leak through the iron or lead pipes to some metallic surface upon which the ear could be placed, had long been known, and in isolated cases, where the existence of a leak had been suspected, this method had been practiced. Moderate quiet is necessary for this performance, and moderate quiet in towns can only be obtained at certain hours of the night. But to apply such a method as a system had hitherto been properly regarded as impossible, because of the necessity it involved for supervision of a kind which has never been found practicable. It was obvious, however, that if, by any means, such a method could be systematically adopted and maintained—results unknown before in connection with the detection of leaks would accrue. If, for example, men at moderate wages, instead of going from house to house during the day, and finding merely a visible defect in, perhaps, every tenth house, could be sent out in the dead of night, with stethoscopes, and with means of access to metallic communications with the mains and pipes, sufficiently close together; and if a record of their success or failure could be made by an instrument beyond their control, the certainty of success would be as great, at least, as the certainty with which a tell-tale clock keeps a watchman awake.

Stop-cocks upon the house-service pipes provided the metallic communications accessible from the street; they also provided a ready means by which a flow through any one of the house-service-pipes, to waste, could be easily shut off. This shutting off produced an instantaneous change of flow in the main, and all that was necessary, therefore, was to devise and place upon the main an instrument capable of recording by means of a diagram, the flow in units of volume per unit of time at each and every instant. Such a diagram cannot be produced by adding clockwork and pencil to any integrating meter, positive or inferential, except by the employment of complex mechanism, but it can be obtained by an instrument of a simpler and totally different kind. This waste-water meter, in its most recent form, is so connected with any water main that the whole of a supply to a population of 1,000 to 4,000 persons passes through it, and that, without any loss of pressure measurable by ordinary pressure gauges. A diagram is drawn automatically, in which the rate of movement of the paper past the pencil is clearly shown. The paper is prepared with vertical hour lines, and with horizontal quantity lines, and is readily fastened to the drum of the instrument. Such a diagram shows in gallons per hour the rate of flow at any time of the day or night. It shows by the perfectly horizontal line, at some time of the night, a perfectly uniform flow, caused by water running to waste, and the varying flow of water caused by use and misuse, distinguishing it from the waste by the varying line.

Mr. Deacon then explained the modes in which the waste-watermeter system is employed in practice, taking as an example the case of a town containing 100,000 persons. The number of waste-water meter districts into which such a town could be conveniently divided would depend entirely upon the arrangement of the water mains, but it would probably be fifty or sixty. Upon the main supplying each such district, a waste-water meter is placed in such a manner that the whole of the water supplying that district passes through the meter. If in such a town, any system of inspection whatever has been adopted, there are probably not less than three inspectors. If they are fairly intelligent men, they may be retained, and no more will be required.

Having fixed the meters and outside stopcocks upon the house service pipes, if such stopcocks do not already exist, all ordinary systems of inspection are at once set aside. One inspector fixes blank diagrams at the rate of 20 a day, and brings to the office as many diagrams from meters upon which they may have been placed from one to seven days before. In a few days from the commencement of the work, the manager has before him the whole 60 diagrams, with the waste per hour visible at a glance, and with the waste in gallons per head, entered by the inspector or a clerk on the diagram in the space left for the purpose. He finds that out of the 60 districts the diagrams show that in 10 the waste per head is five times as great as in 10 others, and that without any reason by which any divergence might have been anticipated.

Instead of wasting the energy of his men upon all the districts in rotation, the manager now concentrates his attention upon the most wasteful 10, and with the worst of these he begins his work. Two inspectors receive orders at night to visit that district, and, in order that they may confine themselves to the right blocks of houses, and omit none, they are provided—in Liverpool, at least—with a small plan of the district, showing the houses supplied through the meter in question. Having reached the district between 11 and 12 o'clock, P.M. one of two methods is adopted.

By the first and most general method, the stopcocks are sounded in rotation, by using the ordinary stopcock turning key as a stethoscope, and any stopcock through which water is heard to be running is shut off, the time and number being noted by the inspector. The shutting off and time of shutting off are simultaneously recorded by the meter on the main, to which the inspectors have no access. On the pavement, above each stopcock so closed, the inspector marks a cross in chalk. If after closing a stopcock the sound continues, it is obviously caused by waste from the main, or between the stopcock and the main. It is then generally heard at several stopcocks, and by its relative loudness at each, an approximation is made to its position. The footway and the carriageway pavements are then sounded, until a spot of maximum noise is found. Here, again, a chalk mark is left, which rarely fails to show the position of a burst pipe or ferrule to the day inspector, who, with his laborer, visits the district on the following day. At the end of two to four hours, the round of the whole district has been made, and the inspectors find themselves again not far from the meter. They next close the main stop-valve, near the meter, and leave it closed for a minute or two. Commencing with this valve, they then reopen all the closed stopcocks, which are readily seen by the chalk marks, and return to the night office, where each inspector writes in copying-ink on the left-hand side of a book the particulars of his inspection. By the second method—rarely necessary except when waste has already been very much reduced—the whole of the stopcocks are at first shut off without sounding. On the return journey, they are opened one by one, and sounded; the result is obviously to magnify the sound resulting from small leaks, supplied by cisterns with ball-taps. At 9 o'clock on the same morning the day inspector receives a press copy of the night inspector's reports. He visits those premises, and those only, in or under which waste is reported to be actually taking place, and the work of many days' inefficient house-to-house inspection is efficiently performed in one. On the same evening he writes in red ink, opposite the night inspector's report, the result of his examination. He also issues the necessary notices for repairs or renewals. On the same day the manager or his clerk receives and records the meter diagram from the district in question. He sees by that diagram the time during which the night inspectors were continuously engaged, and he sees the exact amount of useful work performed in that time. It would not be possible, even if the inspectors had access to the meter, to elude this knowledge. He sees, moreover, the total quantity of waste detected, and one month hence he will see by another diagram the result of the day inspector's efforts to stop it.

The secret of the success of this system, as compared with that of house-to-house inspection, is due to the facts: 1. That the inspectors are always working in the most wasteful districts. 2. That the time occupied in inspection is greatly shortened. 3. That the hidden as well as the superficial waste is detected.

That the time occupied in inspection is greatly shortened may be shown as follows:

Under the ordinary system of house-to house visitation, one man, in one day, can inspect, on the average, the dwellings of about 180 persons. Under the waste-water meter system, the wages paid to him generally suffice for the thorough inspection of the premises occupied by more than 1,000 persons. The invisible as well as the visible waste is detected. This invisible waste frequently exceeds, on the average, one-half the whole waste, and where a thorough house-to-house inspection, occupying a given number of men a given time, and detecting a given quantity of waste, is

followed by an inspection under the waste-water meter system, it is generally found that the same number of men suffice to detect two to three times the volume of waste in one-fifth the time. When, in conjunction with this fact, we take the additional advantage to the latter system of having the inspectors always engaged in the most wasteful districts, its relatively high efficiency is sufficiently obvious.

Whatever results have been obtained by any other method can be brought about by the method advocated by the author, much more cheaply, both to the water authority and to the householder, and with far less trouble and annoyance to all concerned. This system has been applied within the last nine years to districts containing about 1,700,000 persons.

The mode of preventing waste when detected is not affected by the manner of its detection. It will be agreed on all hands, that when it is decided to replace a fitting or pipe, that fitting or pipe should, like those to be used in new premises, be of the best possible kind, and should be fixed and adjusted in the best possible manner. The soundness and efficiency of water fittings can only be conclusively determined by taking each to pieces, examining each part in detail, and finally testing the whole under pressure. Such an inquisition finds defects in a certain proportion of the fittings made by firms even of the highest and most deserved repute.

The fittings used in Liverpool are such as encourage, rather than discourage, the proper use while preventing the waste of water. No pea ferrules or other obstructions to the flow of water are permitted; no taps in which the duration of flow is limited are required, except for out-door stand pipes; and water-closets are not allowed to have new cisterns providing a flush of less than two gallons.

The respectable local plumbers have been invited to sign an agreement to conform to the water regulations issued by the Corporation. The incentive to them to do so is the advertisement of their names on the backs of the waste-water notices. A plumber's name may at any time be erased if he fails to comply. In practice, it is found that work is rarely performed except by men whose names appear in the list, and that there is, therefore, no sale except for fittings tested and stamped by the proper officer of the Corporation.

The cost of adopting the method advocated has always been insignificant in comparison with the value of the results obtained, and is generally entirely covered by the saving of water in from six to 12 months.

THE NEW EDDYSTONE LIGHTHOUSE.

"The Nautical Magazine."

On Thursday, the 18th May, the new tower, which, during the last three and a-half years has been in course of construction upon the Eddystone reef, was formally commissioned by H.R.H. the Duke of Edinburgh. The ceremony was attended by the Trinity yachts *Galatea* and *Siren*, having on board the Deputy Master, Sir Richard Collinson, and many of the Elder Brethren and officials of the Trinity House, as well as sundry distinguished visitors; the Admiralty vessels *Vivid*, *Trusty*, *Perseverance* and *Carron*, took out the Mayor and Corporation of Plymouth, and the authorities of Davenport and Stonehouse. In addition, a number of steamers brought out the general public, and the scenes, both as the flotilla steamed out of Plymouth Sound, and as the numerous vessels grouped themselves around the Eddystone reef, was singularly picturesque. The weather was brilliant, there being just sufficient wind to impart a lively motion to the water, and a general appearance of briskness and vigor to the scene at the rock.

The history of the proceedings in connection with the new tower may be briefly stated as follows:

In 1877 it was determined in consequence of the undermining of the rock, on which Smeaton's tower was built, to erect a new tower, the old building being

at times subject to tremors and vibrations of a somewhat alarming nature.

After several careful surveys, a suitable base for a new tower was found on a rock at a distance of 40 yards from the old lighthouse in a S.S.E. direction, the only drawback to the selected rock being that its top is only just above the level of low water, and the foundation therefore had to be laid below the level of low water. The design of the new tower and the general arrangements in connection with the organization of the staff and direction of the work were left entirely to Mr. James N. Douglass, the Engineer-in-Chief of the Trinity House.

The personal superintendence of the work was entrusted to Mr. T. Edmond, who possessed considerable experience in lighthouse building, and Mr. W. T. Douglass, the son of the engineer-in-chief above mentioned.

In the winter of 1877 and spring of 1878 the preliminaries were all arranged, and on the 17th July, 1878, the first landing on the rock was made, five others being made before the month was out. The first necessity was to build a coffer dam for the protection of the men while working, and to excavate, cut and bench the rock so as to prepare it for receiving the foundation courses. With the exception of a few small stones being carried away in October, the season was a successful one, and was prolonged until 21st December, when operations were suspended for the winter, about one-fourth of the protecting coffer dam having been completed, and 1,500 cubic feet of rock excavated; 40 landings having been made, and 129 hours of work accomplished.

It should be mentioned that this period, while the men were working below the level of low water, was the most perilous. Not more than three hours at a time could be spent on the rock by the working party. From about three-quarters ebb to quarter flood tide was the utmost limit of their stay, and during that interval the utmost energy of all had to be exerted. With a rough sea, landing on the rock was simply out of the question, but often when at work, the party having perhaps effected an easy landing, the sea would get up, and then it would be necessary for all to seize their tools and hurry off to the boats as quickly as possible. Delay would probably mean being hauled off through the water, for no boat could venture near the rocks while the seas were breaking upon them.

The urgency for the construction of the coffer dam was so great that every nerve was strained to complete it. Work was even carried on on Sundays, when fair weather and a good tide offered.

In 1879 the first landing was made on the 24th February, and work proceeded rapidly. The coffer dam was completed by June, and then the shears, winches, &c., were set up for landing the stones. The method of carrying on the work may be briefly described as follows: The twin screw-steamer *Hercules*, employed in carrying from the work-yard at Oreston to the rock the material for the new tower, could carry 120 tons of stone, &c., and occupied a little more than an hour in making the passage. On each day, when there was a fair prospect of landing on the rock, the *Hercules* left Plymouth in time to arrive at the Eddystone reef soon after the beginning of ebb tide; on arrival she was warped into a position a very short distance from the rock, and made fast head and stern. In this position the vessel would be only about 30 or 40 feet from the rock. On the deck of the steamer a railway was fitted, on which a truck conveyed heavy loads, such as blocks of granite, bags of bricks or sand, and barrels of cement, to the stern of the vessel, whence they were carried to the rock by means of a double chain extending from a strong timber framework on board, to the crane on the rock, and worked over the pulleys by one of the powerful steam winches of the steamer. By this plan a three or four ton stone could be hoisted with comparative ease from the ship's deck up to the required height, and then dropped into its prepared place.

On the 19th August, 1878, H.R.H. the Duke of Edinburgh, Master of the Trinity House, in the presence of H.R.H. the Prince of Wales, Admiral Sir Richard Collinson, Deputy Master, and many other Elder Brethren of the Corporation, laid the foundation stone of the new tower. After this the work sped along and the season closed on 19th December with eight courses laid. Strange to say, on the 21st and 22d November the men

worked on the rock for several hours by candle light! As many as 131 landings in the rock were made in 1879, and 518 hours of work accomplished.

On the opening of the season of 1880 much anxiety was felt as to the effect of the winter storms upon the work which had been left. On the 25th February the first visit was made, and it was found that the iron jib of the landing crane had been carried away, otherwise no damage whatever was done. The setting up of the stones was briskly proceeded with, and the tower rose above the level of high water. The operations were not now quite so arduous; a longer time could be spent on the rock, and landings effected more easily. The masonry of the tower was in this season completed up to the 38th course, 110 landings having been made, and 657 hours of work expended up to the 9th November, the date of the final landing in 1880.

In 1881 the first visit to the new tower was on the 18th February and the hoisting in and setting of stones went on with great rapidity until June, on the first day of which month H.R.H. the Duke of Edinburgh, who, as Admiral Superintendent of the Naval Reserves, was on coast-guard duty in the neighborhood, laid the top stone of the tower. The extraordinary quickness with which the work so far had been executed, more rapidly in proportion to dimensions than any rock lighthouse previously undertaken, is explained by Mr. Douglass as being due chiefly to the special steam machinery and appliances for pumping, rock-drilling, and hoisting materials, &c., with which the steamer *Hercules*, employed upon the work, was fitted.

The tower consists of 2,171 stones containing 63,020 cubit feet or 4,668 tons of masonry. Smeaton's tower contained only 988 tons of stone. The sheer weight of the new tower is probably sufficient in itself to enable it to withstand a considerable force of wind or wave, but in addition to this every stone is dovetailed above, below and on all sides, as well as being joined with cement to the stones adjoining, on a plan which is an improvement of Smeaton's method. In Smeaton's tower four living rooms besides the lantern were provided, but in the new tower there are nine rooms each more lofty and commodious than any of those in the old building. The new tower has a cylindrical base from which the main lighthouse-shaft springs. The advantages of this plan are that the circular ledge formed by the cylindrical base offers great facilities for landing from a boat, and at low water affords a convenient promenade for the keepers. A life-line, which is fixed around the tower just above the level of the platform, might be extremely servicable to shipwrecked sailors, if any such unfortunates succeeded in getting a foothold on the ledge.

Up to a height of 25½ feet above the level of high water, the tower is solid. with the exception of a large water tank let into the solid. The stone of which the new tower is constructed is granite of the best quality, from the quarries of Dalbeattie in Scotland and De Lank in Cornwall, by far the larger quantity coming from the latter. Many of the blocks weighed more than three tons, and were dressed and fitted at the quarry.

The Lantern.—The lantern surmounting this noble tower is a splendid piece of work, constructed by Messrs. Chance Bros. & C ., of Birmingham. It is cylindrical, 16½ feet high (which is higher than lighthouse lanterns usually are made, but this is necessary to accommodate the two burners, one above the other, which are placed there) and 14 feet in diameter. A very careful arrangement for thorough ventilation of the light-room is provided, which is most essential, having regard to the great heat which may at times be developed when the lights are burning. Fresh air can be copiously admitted through valves in the lower part of the lantern, and through a grating in the lantern floor which communicates with open windows in the service room below. The burners are thus plentifully supplied with the necessary oxygen, and streams of cold air, ascending all round near the inner surface of the glass of the lantern tend considerably to check the condensation of moisture on the panes, which otherwise might seriously interfere with the effectiveness of the light.

The, lantern, however, is unimportant compared with the apparatus inside for producing the light. In Smeaton's day the illumination was produced by 24

candles of six to the pound, arranged on a chandelier. No reflector of any kind aided the candle lights, and no provision was made for preventing the rays going in directions where, so far as the seaman was concerned, they were wasted. Early in the present century, however, the candles were superseded by 24 oil lamps with reflectors, by means of which the light was greatly improved, both in regard to its power and its concentrated usefulness. In 1845 again a change was made, the Argand lamp and reflector being disestablished in favor of Fresnel's new dioptric systen, by which one large central flame was employed, the rays from which were magnified and refracted (*i. e.*, bent in the direction required), by means of an arrangement of lenses and prisms surrounding the light at a distance of two feet or more on all sides, in form of a beehive. This apparatus, with a four-wicked lamp, has remained in operation until now, but the light in the new tower is of a vastly more important description than those which have preceded it in the old tower.

In speaking of a lamp having four wicks, it should be explained that these four wicks are concentric, or they may be described as four tubes of wick, the larger encircling the smaller ones, the innermost being about one inch, the outermost about three inches in diameter. When burning all the four wicks are alight and yield a fine body of flame. Of late years Mr. Douglass has caused the intensity of the flame to be greatly increased by the addition of two more wicks of proportionately larger circumference than the outermost wick of the four-wick burner.

Two of these six-wick burners are fitted, one superposed on the other, the vertical distance between the two being about 6½ feet.

For ordinary purposes the upper lamp only will be used, the value of the light being 722 candles; with both lamps burning, the combined illuminating power is said to be equivalent to a quarter of a million of candles, or about six thousand times the intensity of the original candle-light of Smeaton's time. What effect this enormous mass of light concentrated into flashes will have upon thick fog remains to be proved, but there can be little doubt that in misty, hazy, slightly foggy, rainy or snowy weather, the flashes will be serviceable to the mariner at distances to which the old light could never have reached, even had it been of the same elevation as the new light.

Although in 1859 Mr. J. W. D. Brown provisionally protected an invention, the main feature of which consisted in the employment of two or more tiers or rows of lenses superposed with a separate light or set of lights for each tier or row, yet to Mr. J. R. Wigham, of Dublin is due the credit of having first practically utilized this idea, with his biform, triform, and quadriform gas apparatus. He employs two, three or four sets of gas burners superposed, each burner consisting of several rings of flame produced by concentrically arranged gas jets, the value of each burner being augmented by a glass dioptric apparatus. These superposed lights yield a splendid effect when in operation, as at Galley Head, on the Irish Coast.

The glass apparatus at the Eddystone by which the effect of each burner is augmented and economized consists of a twelve-sided drum, each side, also called a panel, 6 ft. 3 in. in height and 1 ft. 8 in. in width, being formed by a central lens, or, as it may popularly be called, a bull's eye, and surrounded by concentric rings of larger bull's eyes, by which the same effect is obtained as though a portion of one huge lens of great thickness and weight, as large as the whole panel, was employed. For purposes which will presently be apparent, the two bull's eyes of the adjoining panels are brought close together, very much as though they were two eyes squinting, so that only lengthways they are in the middle of the panel. On the rotation of this twelve-panelled drum, with the inside central light burning, each bull's eye with its surrounding rings carries round a concentrated beam of light, which becomes visible to the outside observer as soon as by the rotation of the apparatus the focus of the bull's eye falls upon him. Now two bull's eyes are, as have been stated, brought close together, so close indeed that a small portion of each is cut off, consequently a very short interval occurs between the flash of the first and that of the second reaching the observer; thus

it will be seen the two flashes occur in quick succession, and then nearly half a minute elapses before another pair of squinting eyes come around and discharge their two flashes. This description applies to one light only; with the two lamps one over the other, two drums superposed are employed, one for each light, the two being identical in all respects and arranged so as to coincide exactly with each other. The height of the whole apparatus is consequently 12 ft. 6 in. and with both lights burning a magnificent effect is obtained. The optical apparatus was manufactured at the works of Messrs. Chance Bros. & Co., of Birmingham, the calculation of all the angles of reflection, &c., being made by Dr. Hopkinson, F.R.S., a work which it is essential should be done with the highest degree of accuracy, in order that the lenses and prisms may be so adjusted as to intercept the rays of light proceeding from the lamp, and bend them so that they go out seaward in the desired direction.

ENGINEERING: PAST AND PRESENT.

Address of ASHBEL WELCH, President of the American Society of Civil Engineers, at the Annual Convention at Washington, May 16th, 1882.

I DO not propose this evening to undertake any general survey of the engineering field. For such a survey, I refer you back to Mr. Chanute's address of two years ago. I shall not attempt to glean after him. But I shall speak of several disconnected subjects of present interest, and give some reminiscences showing the contrasts between the past and the present; and in such reminiscences I shall disinter the buried memories of some of the great engineers of the past.

When we look around on the engineering works recently completed, or now in progress or in contemplation, the first thing that strikes us is their extraordinary magnitude.

Prominent among them is the St. Gothard tunnel, passing for 48,900 feet, or more than nine and a quarter miles, through the base of the great Alpine chain which has hitherto been so formidable a barrier between southern and central Europe, a thousand feet below the vale of Urseren and the villages of Andermatt and Hospenthal, and 6,500 feet, or a mile and a quarter below the eternal snows that cover the crest of the mountain. The cost was about $12,000,000, or nearly $250 per foot lineal. This tunnel is nearly 9,000 feet, or a mile and two-thirds longer than the Mt. Cenis tunnel, by far the longest previously built.

Such stupendous works have been made practically possible by the compressed air drill, and the high explosives now used. In my active engineering days, rocks were drilled for blasting only by the power of human muscle, either by one or two men churning a hole in the rock with a heavy rod some six feet long, or by one man holding and slowly turning a short drill, and another man driving it into the rock with a sledge hammer. Then came the steam rock drill, then the compressed air drill. The compressed air not only does the work, but it ventilates, and its sudden expansion cools the tunnel or the mine where it is used.

The first, or one of the first tunnels in this country in which the rock was drilled by compressed air, was the Nesquehoning, by Mr. J. Dutton Steele. Since then many have been made by the same means, one of the most memorable of which is the Musconetcong tunnel, a mile long, made under the direction of Mr. Robert H. Sayre. This difficult work gave occasion for the valuable treatise on tunnels by Mr. Drinker, who was in immediate engineering charge of it. The Hoosac tunnel, 24,000 feet long, after a long continued struggle, was completed several years ago, and is now in use.

Among the tunnels now being constructed is one half a mile long under the plateau of West Point, and another 4,000 feet long through the hard trap rock of Bergen Ridge, at Weehawken; both on the line of the road now in con-

struction on the west shore of the Hudson. Nearly all the debris from the latter is raised through shafts.

The project is now under serious consideration of making a tunnel some 21 miles long under the straits of Dover. A few years ago such a project would have received only a laugh of incredulity.

The admiration of the world has not yet abated for the boldest of arched bridges yet built, that over the Mississippi at St. Louis; with its steel arches of 500 feet span, its piers of heavy masonry sunk to solid rock more than a hundred and thirty feet below the high water surface of the river, through shifting sands, and during the most fearful floods.

The Brooklyn Bridge, 1,595 feet, or nearly a third of a mile long, over an arm of the sea more crowded with commerce than any other in America, and high enough to allow a line of battle ships to sail under it—is drawing to completion, and will be (though perhaps only for a few years, 'till something more stupendous comes), one of the wonders of the world.

Probably the boldest plan for a bridge ever proposed, is that now in contemplation over the Forth at Edinburgh, but of which it is yet premature to speak.

Many very long spans and important bridges are now in progress in this country, such as the one over the Missouri by Mr. Morrison, but time does not permit even a glance at them.

We are now so familiar with the success of suspension bridges for railroads, that we can hardly realize the almost universal disbelief in that success before they were tried. The late John A. Roebling told me before his bridge was finished, that Robert Stephenson had said to him, "If your bridge succeeds, mine is a magnificent blunder." And yet, unexpectedly to the best engineers in the world, the supension bridge over the Niagara answers the purpose quite as well as the tubular bridge over the St. Lawrence.

The mention of the St. Lawrence reminds us of the great and interesting improvement of that river now going on under the direction of Mr. Kennedy. The original low water channel between Quebec and Montreal, had, in places, a depth of only 11 feet. Now they are increasing the low water depth to 25 feet, with a width of 300 feet. The work is done with bucket and chain dredges, exceedingly well adapted to the purpose. Some of the buckets are armed with great steel teeth which excavate the sold rock (geologically Utica slate, but compact rather than slaty in its structure), detaching and bringing up blocks sometimes containing several cubic feet.

If anything of the kind could astonish us in this fast moving age, it would be the rapidity with which, during the past half dozen years, the construction of elevated railroads in New York, and to some extent elsewhere, has gone on. It is of little use to find their aggregate length, for in a few weeks any such estimate must be corrected. There may now be about thirty-three miles of such roads, all double track. The average cost, including stations and equipment, has been about $800,000 per mile.

One of the cases in which a new contrivance effects a great revolution, is that of the elevator. This has been in use for perhaps a quarter of a century at the Continental Hotel in Philadelphia, and in a few other places, but is now coming into general use, and is revolutionizing the mode of building in our great cities, especially in New York. A block of buildings is not now extended along a street as formerly, but is set up on end, and a highway to the different houses or parts of the block, is not horizontally along the sidewalk, but vertically through the elevator shaft. Sky room is cheaper than earth room. It is said that a lot on the corner of Wall and Broad streets was recently sold for over $320 per square foot, or at the rate of $14,000,000 per acre! Equal to the surface covered with silver dollars 5 deep. These stupendous buildings will give engineers and architects much to look after in the way of foundations.

This reminds us of the Holly plan, in limited use elsewhere for several years, now going into extensive use in the city of New York, of dispensing with private fires for heating, and private boilers for generating steam; and furnishing heat and steam power for a considerable district from one great central set of boilers, piled boiler over boiler, tier on tier, for 120 feet in height. This is one of the operations most characteristic of the

present time. Nothing is to be done now by the individual, but everything by some institution, or corporation, or central power, or great firm. Man has ceased to be a unit, and become only an atom of a mass. With the disappearance of the things themselves, the dear old phrases "family fireside," and "domestic hearth," are rapidly disappearing.

Mr. Shinn and the Engineer, Mr. Emery, have kindly given me some particulars respecting this transportation of heat and power, but I can only refer to one or two points. The first and most obvious necessity is to prevent the escape of the heat. This is done by enclosing the steam-carrying pipe in a small brick tunnel, with a flat cover on the top; and filling the space around the pipe, from the bottom of the tunnel to the flat covering above, with mineral wood, which is found to be an excellent non-conductor. It is made by blowing a jet of steam into a stream or jet of melted furnace slag. The arch and covering of the tunnel are plastered over with asphaltum, to exclude all moisture. The loss of heat is said to be very small. One of the great difficulties comes from the expansion and contraction of the pipes, the range being more than an inch in a hundred feet. This is provided for by making the end of each section of about 80 or 100 feet, terminate in very flexible diaphragms of thin copper, the diaphragms being supported by stiff iron ribs.

Among the great enterprises in contemplation, is the interoceanic canal, or the interoceanic railroad for large ships. This is not the occasion for expressing any opinion on any of the competing projects. I will only say that if the world is determined to have a sea level canal, it makes a great mistake in not getting fuller information about the San Blas route.

Many things that have been done by this generation seemed beforehand far less possible than the successful working of the ship railway proposed by Captain Eads. The difficulties are certainly very great, but we can see how they may be overcome. The real question is, whether taking into account the expense of overcoming those difficulties, the construction and operation of such railway will be more economical in the end than the construction and operation of some one of the proposed canals.

The last year has been one of intense activity, particularly in railroad construction. A year or two ago money was so abundant, and, therefore, interest so low, and so many capitalists, great and small, were tired of letting their money lie idle, that new enterprises of many kinds were started, especially new railroads, and enlargements of capacity of those already in use. As the money market has approached its normal condition, some of the new projects have been dropped.

It is instructive to look back and trace the connection between the progress of railroads and the financial condition of the country.

From the year 1787 there has been a financial catastrophe, or at least depression, in our country regularly every ten years down to the year 1857. The cause of this seems to be rather psychological than anything else. It seems to have taken the American business mind just ten years to pass through the various stages and degrees of panic after the financial crash, through extreme cautiousness, moderate cautionsness, moderate confidence, great confidence, extreme confidence, recklessness, and then another crash.

These decennial depressions were modified by circumstances. That of 1817 was intensified by the effects of the war of 1812 and by the failure of the crops of 1816. That of 1837 was moderated by the efforts of the United States Bank, and part of its effects postponed until the final failure of the bank a few years later, which produced the intercalary depression of 1842. The effects of the crash of 1847 were moderated within two or three years by the discovery of the gold in California.* The crash of 1857 was intensified by the previous inflation from the gold excitement, the rapid railroad construction in the West stimulated by the land grants, and its effect continued longer than usual on account, first, of the apprehension, and then the reality of civil war.

The effects of a financial crash do not

* That discovery was first made in digging the foundation or the tail race of Sutor's Mill, by James W. Marshall, who fifteen years before had been a boss on work going on under my direction, and whose three sisters are still neighbors of mine.

appear in the statistics of railroad construction till a year or two after it takes place, for if a road is well advanced towards completion, it will probably soon be finished, even during a panic. This is shown in the statement following.

In consequence of the financial troubles of 1841-2 the mileage of new railroads opened in 1843 and 1844 fell off 71 per cent. below that of the two preceding years. Before the panic of 1847 had time to reduce the increase of mileage its effects were more than counterbalanced by the discovery of gold in California and by the land grants, After the great crash of 1857 the new mileage in 1859 and 1860 fell off 57 per cent. below the average of the three preceding years.

During the four years of the war the new mileage was 64 per cent. less than that of the four preceding or of the four succeeding years.

Notwithstanding the excitement and inflation after the close of the war, the periodicity of the financial intermittant was broken, and no crash occurred in 1867. The causes are too recent and too well known to require mention. Besides the influx of money from the sale of our government bonds abroad, the ocean telegraph hastened the equalization of interest on both sides of the Atlantic, and the flow of money to the points where it was wanted. A few years ago the normal rate of interest in the West was 50 per cent. higher than in the East. Now there is but little difference. The depression was postponed till 1873.

From the close of 1867 till the close of 1874, when the effects of the panic of 1873 became visible in the statistics of railroad extension, more than 4,400 miles of railroad per annum were opened, twice as much as the yearly average of any similar period had been before. For the next three years (1875, '6 and '7) the annual increase fell off 69 per cent. below the average of the preceding seven years.

The troubles that followed the panic of 1873 were entirely different from those that followed any of the decennial or other panics previous to that time. They were financial; this was commercial. In all the earlier cases the difficulty was want of money, in this last case there was, or soon came to be, a plethora of money. Those were convulsions, this was stagnation. There were more means of production and of transportation than there was demand for. If wealth consists of such means, then the community were suffering from excess of wealth.

The railroads opened in the United States January 1, 1880, aggregate 86,500 miles in length, being 40 per cent. of all the railroad mileage of the world. Last year we had 93,600 miles, and this year we have just about 100,000 miles. But mere length is a very inadequate measure of their magnitude. The terminal mile of some roads has probably cost as much as five hundred miles of some other roads. At one time, and possibly now, the cost per ton taken, on the first two miles of the road from New York to Pittsburg, was more than the cost of carrying that ton over the next two hundred miles. The increase in aggregate magnitude of all the roads may be almost as much in the enlargement without increase in length of the old, as in the extension of the new. We hear in more than one case of thirty miles of additional terminal tracks being laid at one point.

The diminished plethora of money, and the greater caution now apparent, will, it is to be hoped, moderate the increase of the means of production and transportation beyond the demands of consumption, so as to prevent another stagnation.

The investment in railroad property in the United States is set down at about 5,000 millions, perhaps about one-eighth of the value of all the property of the country, real and personal.

When we speak of the extraordinary magnitude of the engineering works of the present day, we do not forget the pyramids, temples, and fortifications of Egypt and Chaldea. Some of them exceeded in magnitude anything that has been made since. What makes it more strange is, that the force that produced them was almost entirely human muscle, while now the work is done largely by steam directed by human brain. Two contrasts strike us as we look at the ancient and modern: the one was executed by slaves and conscripts, with little or no compensation; the other by free men, glad to work for the compensation offered. The old was for the glorification of the few; the modern for the use of the many.

The stagnation that followed the break-

down of 1873, and the consequent low rates of transportation, compelled the managers of railroads to reduce the cost to a point previously thought unattainable, by increasing the power of the engines and the weight of the trains, by more convenient arrangements, by more service of the machinery, by cheaper construction and repairs, by better machinery and organizations of labor, and many improved appliances for handling, and by the stoppage of leaks generally.

American engineers and managers have often shown that *poverty* is the mother of invention. For example, they used cross ties as a temporary substitute because too poor to buy stone blocks, and so made good roads because they were not rich enough to make bad ones. American engineers are, or at any rate, were trained on short allowance of money. As that is the best engineering which accomplishes the purpose at the least cost in the long run, American engineering ought to be of the best.

It is doubtless the fertility of resource coming from the necessity of effecting much with little means, which has created a demand for American engineers in other parts of the world. A few years ago the Government of British India sent for an American engineer, and the first thing they asked him to do was to report on their railroads from the American point of view. Our lamented past president, W. Milnor Roberts, was employed by the Government of Brazil, as I judge from what happened after he went there, to train their engineers, educated in European schools, in American modes and ideas.

Among the greatest of the projects of the present day is the improvement of the Mississippi River.

Towards it the eyes of our profession and of the whole country have of late been anxiously turned. It has overflowed an extent of territory of more than 20,000 square miles, and destroyed millions on millions of property and hundreds on hundreds of lives. One of the most important engineering problems of the age is how to restrain its ravages, as well as to improve its navigation.

In order better to understand what the Mississippi River Commission is doing for these purposes, let us glance at a few of the principles which, or some of which, doubtless control the action of that commission. Those principles are very simple, though their application is often very difficult.

The quantity of solid matter of greater specific gravity than water that a running stream is capable of carrying in suspension, other things remaining equal, increases with the increase, and decreases with the decrease, of the velocity of the stream. Like most cardinal principles, this is so simple and obvious that it seems ridiculous to state it.

It follows, from this, that when a stream is loaded with such matter up to its carrying capacity, then, other things remaining the same, if the velocity is decreased, it will drop part of its load, and if the velocity is increased, it will, if suitable material is in contact with the current, take on more load.

Mathematicians have calculated that the difference in velocity between parallel films of moving water keep the particles of solid matter afloat; but, as is obvious to the eye, and as Mr. Francis has proved, running water does not move in parallel films, and it is also obvious to the eye that the suspended matter commonly moves more or less up and down. The real motion is a compound of parallel and ricochet movements, combined in all sorts of ways and proportions, the boiling and plunging movements increasing with the velocity, the unevenness of the bottom and sides of the channel, and the presence of foreign objects and aquatic vegetation, and being greater in proportion to the whole volume of the water when that is shallow. It is largely this boiling movement which raises the solid matter and keeps it afloat. With the same velocity, the greater it is, the greater the capacity of the stream to carry such matter. Some of the causes, however, which produce the boiling motion may diminish the velocity, and so, on the whole, diminish the transporting capacity.

This is one reason why the exact relation between velocity and transporting capacity is so difficult to determine.

The same current will raise and carry a greater weight of small than of larger particles of the same form and material; for the impact of the current against the particle, tending to move it, is as its surface, that is, as the square of its linear

dimensions, while the weight and consequent resistance to motion is as the cube of the same dimensions. Flat particles are carried more easily than round or cubical, for they have more surface in proportion to weight. Of course a particle of greater specific gravity, as of trap rock, is harder to move than one of the same form and size of less specific gravity, as anthracite. It takes eight times the force to raise a particle of specific gravity 3. in water, that it does to raise one of the same size of specific gravity 1½. This shows why, in many cases, a higher velocity carries no more weight of solid matter per cubic foot of water than a lower; the higher velocity and greater boil take up larger and heavier particles than the lower, and a much larger amount of transporting capacity is used up in carrying them than in carrying an equal weight of finer and lighter particles.

This is another reason why the exact relation between velocity and transporting capacity has not been ascertained; the sizes and specific gravity of the particles transported are not known, and therefore their effect on total quantity transported is not known.

This relation might perhaps be found by some such experiments as the following: 1st. Grind some suitable kind of stone of uniform substance to fine powder; then, by sifting, separate the particles of the powder or dust into lots according to size, each of uniform fineness; then see how much weight of each of these sizes per cubic foot of water can be carried in suspension at the same velocity. 2d. Make the same experiment with stone of different specific gravity, sorting it into lots of the same sizes, the water being kept at the same velocity. 3d. Try the same things with different velocities. The facilities for doing all this can probably be found at some cement mill.

The specific gravity of the bank furnishing the silt, or of the bar formed by it, or of the sediment deposited from the water, gives no information of the size of the particles, and little of their specific gravity. Hence the transporting power with the same velocity appears so different in different observations. Total weight gives only partial information.

I should expect that the transporting power would be as the square of the velocity. I have washed out bars of heavy sand by temporarily confining the current over them, and its power of removing the sand seemed to be about as the difference in level of the water above and below, that is, as the square of the velocity created by that difference.

Though the weight of solid matter per cubic foot of water carried near the bottom is often but little more than near the surface, it is commonly much coarser, and therefore uses up much more transporting capacity. The velocity near the bottom is also less. From each of these circumstances, especially from both together, it follows that the transporting capacity is much greater near the bottom, where the boiling motion is greatest, and where the difference in the velocity of the films of water is the greatest, than near the surface.

It is sometimes said that the transporting capacity with any given velocity is inversely as the depth. This cannot be so, for it would lead to the absurd conclusion that, with the same velocity, a stream a foot deep is capable of carrying as much silt in the aggregate as a stream a hundred feet deep.

If a stream runs over a soft uniform bed for a sufficient length of time, it will become charged with the maximum quantity of solid matter due to its velocity, its depth, its boil, and to the size, shape and specific gravity of the particles taken up by its current. If there is not suitable material within reach of its current, it will carry less than its maximum. As before pointed out, aggregate weight of silt alone is a very imperfect measure of transporting capacity. The maximum load with the same velocity may perhaps be two or three times as great with one material as with another.

If a stream carrying its maximum quantity of silt widens as you go down stream, so that, when the water is high, its section becomes greater than that of the stream above, the velocity decreases there, and a deposit takes place. The coarsest particles will drop first, and thus the bar formed is likely to be hard. When the water subsides, so that the area over the bar becomes less than that of the deeper water up-stream, the declivity of the surface must be increased in order to get the increased velocity

necessary to pass the water through the smaller area, and that raises the surface above the bar, deadens the current upstream, and causes a deposit to take place in the deeper water above. Thus the tendency of expansions of a stream beyond its normal width is to raise its bottom not only there but everywhere, and consequently to increase the height of its floods.

If, on the other hand, a wider place is contracted to the normal width of the stream, the velocity will be increased so as to cut out the bar, if the material of which it is composed is not too hard. By making the channel of uniform width, and keeping it regular and even, the bed, if soft, will be lowered, and the height of floods diminished. With a given discharge, the greater the depth, the less the fall required; or, with the same fall, a less area. A memorable example of the deepening effect of the contraction of a stream to the regular width is by the South Pass Jetties.

The tendency of the greater velocity to take up and carry off solid material is illustrated at bends of rivers. The swiftest water is near the concave shore, that side of the channel is in consequence deepened, and the more rapid current eats into that shore. The current on the convex side is slackened and a deposit takes place. Hence a crooked stream has a constant tendency to become more crooked.

It has always been a wonder why an eddy current was more erosive than a direct current. My theory is, that when the water turns from its direct course and curves round towards the shore, the centrifugal force separates and throws off a a part of the coarser particles held in suspension (just as in old times when a farmer threw a shovelful of mixed wheat and chaff, the heavier wheat went beyond the chaff), and thus the current being now deprived of a part of its load, its power of erosion is partially restored, and it cuts the bank rapidly.

The Mississippi River approximates the conditions of such a stream as I have described.

The first thing done to improve it is to make its channel as uniform as possible by contracting its wide expanses. This is done by placing a continuous line of brush mattresses or screens along each boundary of the modified channel, the edge of the mattress next the channel being sunk to the bottom with stone, the edge farthest from the channel being buoyed up to the surface of the water. The silt-bearing water filters slowly through the mattress, and the current being deadened, drops its sediment and soon forms a sediment under and behind the mattress. This new bank is protected from erosion by the inclined face of the mattress. In floods the current goes over the mattresses into the bays outside, where the velocity being slackened the silt is deposited, the bays are gradually filled up, and dry land ultimately forms between the line of the mattresses and the original shore. Confining the current increases the velocity and deepens the channel between the lines of the mattresses, a uniform channel is established, the bed of the stream is lowered, the water being deeper less declivity of surface is required, the water surface is lowered, and the overflow in floods moderated.

When running water washes the foot of a vertical bank, suppose for example 60 feet high, and washes out a narrow groove along its face, suppose a foot deep, and then the overhanging mass falls so as to leave the bank still vertical, the quantity that falls into the stream is 60 cubic feet per foot lineal of the stream. The finer part of this will be carried down stream, the coarser will probably gradually work down to the bottom and raise the bed. Thus the capacity of the river will be diminished and the height of the surface and of the floods increased. But if the water of the same stream washes a foot horizontally into a bank sloped one to one, and the overhanging weight falls so as to leave the back of the step thus made vertical, the quantity thus thrown into the stream will be only half a cubic foot per foot lineal.

Hence the absolute necessity of sloping the banks of the Mississippi where they are steep and unprotected. The commission are forming this slope by the use of the water jet, and protecting it until the rootlets and willows cover and protect it, by a slight covering of brush.

The great forces of nature, though they cannot be resisted, may often be

guided and controlled by means that seem the feeblest. The magician of science is to control the mighty Mississippi with the willow wand.

If a stream of uniform section, bearing its maximum load of silt, *and confined within its banks*, is furnished with ad additional channel, then though each channel may take its proportion of the silt brought down from above, the reduction of velocity consequent on the increased aggregate sectional area, will cause a deposit to take place below the bifurcation, the bed of the original channel will be raised and its capacity diminished. Hence a bar is likely to form below an extensive crevasse.

But if a stream overflow its banks, then the water that would otherwise run overland may be carried off by additional outlets, so that that they do not lessen the velocity of the main stream, below the point of diversion.

The principles that govern such cases are mostly plain enough, but owing to many disturbing circumstances, their application is often very difficult. A thousand cases may arise where opposing tendencies operate, each tendency with imperfectly known force, about which no man can form an intelligent opinion without an intimate knowledge and careful study of the circumstances, and careful weighing of the force of the opposing tendencies.

I have stated those principles and their application, not because hydraulic engineers will find anything new in the statement, but to bring them to the attention of such dry land engineers as may not already have considered them.

I think no apology necessary for dwelling so long on this subject, for there is no other so opportune, no other more important.

To this generation it seems almost ridiculous to mention turnpikes as ever having been of any interest. And yet the city of Philadelphia retained for a time its commercial ascendency by them, especially by the great Lancaster turnpike. If I rightly remember the language of the geography I studied when a boy, it somewhat exultingly described this turnpike as "seventy-two miles long, four rods wide, and covered, wide enough for two wagons to pass, with eighteen inches of pounded stone." It was over this highway that the wealth of the interior poured into the commercial metropolis of America, in Conestoga wagons.

The national roads from Washington and Baltimore into Ohio, made by the Federal Government, are famous for their share in settling some of the important constitutional questions of our government. One great party disputed the power of Congress to use the nation's money for any such purpose. The contest was long and fierce, but Congress, with much misgiving, made the appropriations. When a few years ago they appropriated $15,000 for the improvement of the Kiskiminitas, they must have got bravely over such misgiving.

Though canal engineering is a thing of the past, its history is instructive. In England it commenced 120 years ago, the first engineer being James Brindley, a millwright. He seems to have known little of what had been done before, and his plans were evidently original. When he proposed to build an aqueduct across the Irwell for the Duke of Bridgewater's canal, his critics said they had often heard of castles in the air, but they never heard before where they were to be put. Brindley built several canals, on one of which was a tunnel a mile and a third in length.

He was succeeded in canal making by such men as Telford and Smeaton and Rennie. Though uneducated, he gained the admiration of scientific as well as practical men. When he wished to study a subject thoroughly, he "laid in bed to contrive," as he expressed it. The secret of his success, therefore, evidently lay in concentration of attention on the subject in hand, and he kept out of the way of anything that could distract his attention.

The era of canal building in England was rather less than seventy years: between 1760 and 1830.

During the last decade of the last century, several efforts were made to connect the detached navigable reaches of some of the rivers in this country, by means of short canals and locks. One of these was undertaken at Richmond under the inspiration of General Washington. Another was at Philadelphia, around the Falls of the Schuylkill. But

the one of special interest in the history of engineering, was at Little Falls on the Mohawk.

The great thoroughfares between the City of New York and the West and Northwest was up the Hudson and through the valley of the Mohawk. The transportation through that valley was partly by three, five, or seven-horse teams over the Genesee Turnpike,* and partly by boats on the river. Those boats were like what on the Delaware we used to call Durham boats, which were 8 feet wide and 60 feet long, drawing, when loaded, a foot or two, and carrying from 10 to 20 tons. They were pushed up stream by two or four men, with setting poles held against the shoulder, and kept on their course by the captain with a long steering oar.

At Little Falls the descent of the river is over forty feet, and, of course, the boats could not pass, but their cargo was carried by the portage of two miles, to other boats above or below. To avoid this the canal and locks were built. They were finished in 1794. Jedediah Morse (father of S. F. B. Morse, of telegraphic fame) published his great standard American Gazetter a few years later, and in it he quotes the following expression of the public sentiment of the time: "The opening of this navigation is a vast acquisition to the commerce of this State." It was conjectured that these locks (which a man could almost jump across), and similar "great works" west of them, might soon make the little town of Albany the capital of a great empire.

The Mohawk continued to be the principal artery of commerce from New York to the interior, until the opening of the Erie Canal in 1825.

Mr. Weston, "that haughty British engineer," as an old gazetteer calls him, was brought over from England to build the locks at Little Falls and elsewhere. One of his assistants was a land surveyor of Rome, New Yoak, named Benjamin Wright, or Judge Wright, as he was called. When, years afterwards, it was decided to build the Erie Canal, Judge Wright, though havlng only the slender experience he had acquired under Weston, was appointed chief engineer. The skill and good judgment which was shown by this father of American engineering, the few errors into which he and his still more inexperienced assistants fell, the great effects produced by them with the means at their command, and the adaptation of their works to the circumstances of the time, are absolutely wonderful.

One of Judge Wright's principal assistants was Canvass White. His skill early brought him into notice, and he was sent by the State of New York to England to learn what he could, especially about hydraulic cement. Despairing of getting it at any reasonable price, and of making it stand the voyage, then from four to ten weeks, he set himself on his return to finding or making a substitute for European cement.

Led partially by the geological position of the hydraulic limes, in England, and partly by what was known of their composition, he explored and tested certain rocks of Western New York, and made the first discovery of hydraulic cement in America. The State of New York gave him ten thousand dollars for his discovery. Subsequently he discovered or recognized cement rock in Pennsylvania in the way till then unknown, but now so familiar, by the contact of limestone and slate.

And yet how soon those men, once so widely known, are forgotten. An eminent and excellent engineer, who had paid especial attention to cement, lately told me he never heard of Canvass White.

One of Judge Wright's assistants, but much younger than Canvass White, was John B. Jervis, whose name to-day is one of the most honored on the rolls of this society.

Many of the distinctive characteristics of American engineering originated with those Erie Canal engineers. We practice their methods to-day, though most of their very names are forgotten. As a class, they wrote little. There were then no engineering papers prepared, and no engineering societies to perpetuate them, if they had been prepared. They were not scientific men, but knew by intuition what other men knew by calculation.

* The migration to the West, (which then meant the Genesee country) was over this turnpike in horses or ox teams; the patriarch of the family and his wife having on their shoulders the same black and white coverlet, and the big brass kettle full of dishes hanging under the hinder axletree of the wagon. Some of their grandchildren now sit in the high places of the nation.

Judge Wright's counsel was "as if a man had inquired at the oracle of God." What science they had, they knew well how to apply to the best advantage. Few men have ever accomplished so much with so little means.

The mention of cement reminds us of quite a new use of it, lately, under the direction of Mr. Chanute. The Erie road crosses the Genesee River by a high viaduct just above a fall. The bed of the river was wearing away, and would soon destroy the viaduct. An artificial bottom of cement has stopped the wear.

The Erie Canal was opened in 1825. Gov. Clinton passed through in a boat on one corner of the deck of which stood a cask of water from Lake Erie, on another corner a cask of water of the Hudson. Gov. Clinton limped from the boat to the public halls, and speeches were made by and to him; and it was a great glorification. The result justified the public expectation. It built up the City of New York, and settled the question of commercial supremacy between that city and Philadelphia.*

The success of the Erie Canal soon brought about the construction of many others. They were thought to afford the most economical means of transportation, and railroads were made, not to carry goods to the final destination, but to a canal or other navigation. After the success of the Liverpool and Manchester Railway in 1830, this opinion was seriously shaken, and in a short time canal construction mostly ceased. Its era in this country was scarcely a quarter of a century, between 1817 and 1835.

Canals to be successful now must be capable of passing vessels of large capacity, must not have too much lockage, and the locks must be worked by steam or water power; the boats must be moved by steam, either on board, when the vessels are large enough, or, when the vessels are smaller, by locomotive on the bank, or by cable at the bottom, and then the locks must be large enough to hold the fleet taken by one locomotive or cable tower; there must be plenty of water, and the canal must connect harbors or navigable waters.

I tried towing by locomotive on the canal bank more than forty years ago. There is, of course, no difficulty in one engine towing several boats, but if the locks are not large enough to pass the whole fleet at once, the delay of all the fleet till each boat is passed separately, counterbalances the economy of steam instead of horse power. The speed even for light boats cannot be increased to more than five or six miles per hour on account of the wave.

Cable towing, notwithstanding the reported failure on the Erie Canal, can, with proper boats and apparatus, and with experienced men, be easily performed on the crookedest canal in America, as it is now done in Belgium.

Canal engineering does not avail itself of the engineering resources of the age. Little improvement is made in it: mainly, I suppose, because it is not considered worth improving.

The most remarkable early river improvement in this country was that of the Lehigh.

About the year 1817, Josiah White and Erskine Hazard commenced the improvement of this river, and made other preparations to inaugurate the anthracite coal trade. In 1820 they sent to market 365 tons, which was the beginning of the regular anthracite coal trade of America. Now the annual amount will soon reach 30,000,000 of tons.

The descending navigation they made consisted, first, in clearing the channel of rocks, and confining the water in the rapids, when low, to that narrow channel by boulder wing dams; second, when the fall was too great for this, in building dams with bear trap locks; and third, in storing the water in pools, and letting it run only when the coal arks were running.

The bear-trap locks have given the hint for several devices since used, and are well worthy of examination. Near each end of the lock was a pair of gates, each gate reaching across the lock and to the back of the recess on each side, which gates, when not damming back the water, lay flat on the bottom of the lock. The lower gate could be made to revolve through an arc of somewhere about 40 degrees around a horizontal axis coincident with its down-stream edge. The upper gate of the pair, when laid flat, lapped over about half of the

* An old pilot once told me that in his younger days there were three or four ships out of Philadelphia to one out of New York.

width of the lower gate, and revolved through a similar arc around its upstream edge. When laid flat, the water, of course, ran freely over them. They were raised by admitting the water under them from the pool above the head of the lock, through the side wall, when the pressure of water pressed them up. They were prevented from going too far by shoulders in the recesses. The gates then came within 10 or 15 degrees of being at right angles to each other, the under side of the upstream gate resting on the upstream edge of the downstream gate. They could be held in any position, so as to hold back the water entirely, or let it run over with more or less volume, as required. The arks containing the coal were commonly shot through over the partly raised gates as over so many dams.

Such locks, copied from those on the Lehigh, are now in use on the Ottawa, at the Canadian capital. Many of us at our last convention were shot through them on rafts.

It is well worth inquiry whether these bear-trap gates would not be the best possible, and possibly the cheapest, for letting the water rapidly out of a reservoir for scouring purposes. A full stream could be set running in a few seconds, and the flow could be regulated with perfect ease, and stopped at any moment.

In many rivers it is desirable to dam the stream back at low water, and let it run freely at high water. In Belgium, on the Meuse, they use needle dams for this purpose. Another probably better adjusfable dam is in use in France. The bear trap gates, with proper appliances, on a solid platform at the bottom of a river, would enable a man on shore to raise a dam across that river, or if raised, to lower it to the bottom, in a few minutes.

I have used this contrivance for a fish sluice in a permanent dam, by which the water ran freely through the sluices when necessary, and at other times was retained at full height.

The coal, on the descending navigation of the Lehigh, was sent to market in arks consisting of six boxes, 16 feet square and 20 inches deep, coupled by hinges, the whole carrying about 100 tons.

Of course, it often happened in that hazardous navigation that the arks were wrecked. The lumps of hard coal were soon rolled down-stream by the current to some shoal below, where they were found in the form of completely rounded boulders.

In making these improvements, eight hundred men were employed at once near Mauch Chunk, then in the wilderness, quite outside of the bounds of civilization. It was not easy to control these men, many of whom, doubtless. had never been remarkable for good order. The sheriff of the county was unable to make an arrest. But the fertile genius of Josiah White, and the strong good sense of Erskine Hazard, soon found a remedy. Under their inspiration the men organized themselves into a republic, adopted a code of laws, which their backwoods poet put into rhyme, and these laws, which they themselves had made, were strictly enforced and universally submitted to. Punishment was inflicted by a good stout hickory stick, as big as your finger, well laid on with a strong arm.

The chief executive of this republic, called the lieutenant, was also the executioner. When all hands were called to witness punishment, they said or sang the part of the law which had been transgressed, and the lieutenant beat time on the offender's back. One of the gravest offenses was for a man to take more on his plate, or his shingle, than he could eat. Punishment of this soon stopped the grabbing, and the provision bills were very much reduced. At any official announcement, the expression of loyalty to the supreme authority, was not as in England, "God save the King," or as in Pennsylvania, "God save the Commonwealth," but "Hurrah for Mr. White and all the rest!"

Engineers and employees may well take a hint from this piece of history.

Josiah White, the Pennsylvania Archimedes, as he was sometimes called, invented, among many other things, the drop gate so valuable in canal locks of moderate rise. In 1827, he and Hazard built the Mauch Chunk Railroad, nine miles long, the first railroad (except a little tram road at Quincy granite quarries) ever built in America. My hap was to ride on it within a few weeks after it was opened.

In the early times of the coal business

the same coal passed in succession through several hands, each of whom had an interest distinct from the rest. The owner of the land, the mine operator, the owner of the lateral road to the canal, the canal company, the boatman, the tide water vessel owner, and the coal merchant, must each make a profit, or he would stop, and that would stop all the rest, though, taken altogether, the profits made by some would greatly counterbalance the losses made by others. Hence, those parties who performed all the operations, succeeded best, for they always kept on and made something, while those who took the different steps of the business in succession were stopped, because some of them made nothing. Thus, the latter were driven to consolidate, though often against their earlier intentions. The owners of coal roads bought large tracts of coal land, not to monopolize, but to insure a constant stream of transportation, at times when private owners are accustomed to stop, because there is no profit in their branch.

This generation wonders how the business of the world ever could be carried on, and especially, how railroads ever could be run, without the telegraph. And yet many of us remember when there was none. And after it was shown that information could be sent by an electric current through a wire, it was years before any one made use of it.

About fifty years ago, Professor Henry made a series of brilliant discoveries in electro magnetism, one of which was, that by means of a current through a wire, a signal could be made and information given (by ringing a bell, for example), a long distance off. Years afterwards, Steinheil, Morse, Wheatstone and others, applied Henry's discovery to the actual conveyance of information; Morse's apparatus, as it seems to us Americans, being by far the best. The wonder to us now is, why Henry himself did not apply his discovery, and why others did not sooner do so. The answer is found in a very important phase of human mind. The habit of mind into which the scientist is liable, perhaps likely, to fall, is to look at scientific result as his ultimate end. Such result arrived at, the same habit of mind is to use it only to attain further scientific result. Hence, men of science so rarely are benefited pecuniarily by their own researches. Hence, also, it frequently happens that engineers who have kept at their studies without practice till too late in life, are so often less successful than those of far less science, and, perhaps, less intellect, but who have been early trained to apply to practical use what science they have.

Iron ship building has had almost its entire growth within the last forty years.

In the spring of 1845, I visited a small iron ship yard, then quite a new thing, at Birkenhead, on the south side of the Mersey. The proprietor, in his green flannel roundabout, showed his modest establishment, and explained some of the processes. That proprietor became afterwards well known to the world as Sir John Laird, the great iron ship builder, and especially to this country as the builder of the *Alabama*. The operations of that enterprising craft came near involving us and our cousins across the water in a very serious conflict. This was averted by the moral courage and enlightened patriotism of Grant and Hamilton Fish on this side, and Gladstone and Clarendon on the other, who, not having the fear of demagogues before their eyes, agreed upon arbitration instead of war. All honor to the statesmen who took this great step in Christian civilization.

They were just beginning to build the first dock wall on the red sandstone bed rock of the Mersey; now they have 159 acres of dock room enclosed. Then Birkenhead was a small village; now it has more than 100,000 inhabitants.

America is not the only country that moves.

Mr. Chanute, in his annual address, two years ago, spoke of the first propeller boat used in America. That propeller fell into my hands; and I towed the first fleet of boats ever towed by a propeller tug on this side of the Atlantic, from Philadelphia to Bordentown, in October, 1839. Now, our harbors are full of them. The first propellers ever built in this country, and, as far as I know, the first iron hulls, were the *Anthracite* and the *Black Diamond*, built on the Plans of Captain Ericsson, and employed in carrying coal through the Delaware and Raritan Canal. The first sea-going propeller built in this

country was the frigate *Princeton*, built on Captain Ericsson's designs, under the direction of Captain Stockton. It was a full rigged sailing ship, the intention being to use steam only as auxiliary.

It should not be forgotten that John Stevens, almost eighty years ago, built a small propeller boat, with two propellers, or "circular sculls," as he called them, and ran it about the harbor of New York. It is wonderful how near his blades approach the angle which experience has shown to be best. He used a small locomotive boiler, as it would now be called, such as was reinvented by Booth, a quarter of a century later, at Liverpool.

The rapid progress of the country, and the activity of the age, are more strikingly shown by the records of the Post Office Department, than by the increase of population—from three to fifty millions since the revolution—or than by any other statistics I know of. During several years of the time that Benjamin Franklin was Postmaster General, he personally kept the whole accounts of the department, and all in one small book, and settled with the postmasters and mail carriers. There were then about, perhaps, twenty or thirty dead letters a year, now there are four millions. It now takes eight clerks constantly employed to open them, and I remember that it takes fifty clerks to take charge of one class of them. Franklin kept one small book, which lasted three years, now there are 150 or 200 books, each half a dozen times as large, filled each year. Then the work was done by Franklin for $600 a year, now by 700 clerks, for, perhaps, a million a year.

Within my memory, some of the sciences with which engineers have specially to do, have grown from infancy into at least adolescence.

For example, geology was a collection of interesting but isolated facts, and unverified theories, now it is a science. It used to be considered terribly heterodox, and a young man who cared to stand well with good people found it safest to say nothing about it. To read geology was next to reading Tom Paine. A learned and excellent divine once confidently informed me that all the supposed plants and animals found in the rocks were merely stones that happened to come out in that shape. Now geology has an important connection with the instruction in theological seminaries.

Business and population depend on geology. A geological map of England enables one to locate its occupations and the denser populations. An outcrop of gneiss, extending southwest from New York, forms the limit of tide in the rivers, and fixes the location of Trenton, Philadelphia, Wilmington, Baltimore, Georgetown, Richmond and other cities to the southwest.

When I studied chemistry at school, the components of compound bodies were given in percentages. For example, limestone was 48 per cent. oxygen, 12 per cent. carbon and 40 per cent. calcium. Of course, nobody could remember such proportions. Nor did it give the proximate elements of the compound. The automatic theory, as it was called, was known, but chemists were cautious about accepting it. They had not yet learned to distinguish between the *theory* of atoms, and the *fact* of equivalents.

One of the most surprising feats of modern science is seen in the daily predictions we have of the morrow's weather. Time was, and many of us remember back to it, when predictions were made, and by intelligent people, too, from the phases of the moon, from weather breeders, from the weather on certain anniversaries, and the like.

More than a century ago Franklin pointed out the fact that northeast storms begin at the southwest, two or three days earlier at New Orleans than at Philadelphia. Much information was afterwards accumulated, and scientific investigations were from time to time made by many able men. About forty years ago Prof. Espy of Philadelphia announced his theory, that rain is caused by the rarefaction and consequent upper movement of the mixed air and vapor into a colder region, where the vapor is condensed and falls into rain, and that this rarefaction produced by the heated surface of the earth, or by fire or otherwise, causes the denser air to flow in from every side, so that the wind blows toward the rain. All this has been since verified. But this sanguine philosopher did not get the credit he really deserved,

but drew upon himself the ridicule of the world, by claiming for his discovery more than it could accomplish, especially by proposing to raise the Mississippi by setting fire to the woods on the Allegheny mountains, when the hygrometer showed much moisture, and so getting the upward current required to make it rain, just as it commonly rains after any great fire, or the eruption of a volcano, or a battle.

Espy visited Princeton to confer with Prof. Henry. I was present at the interview. Henry, while he thought Espy's main principle quite correct, got very much out of patience with him for several hasty conclusions from statements which, to Henry's cautious, scientific mind, did not seem at all conclusive.* After he was gone, Henry chalked out the plan which he afterwards, with the co-operation of Guyot and other able men, so successfully carried into execution, of simultaneous observations all over the country, and a daily chart of highest and lowest pressures, and other things about which my memory is less distinct. As everybody knows now, it is the traveling of these lines from west to east, at an average of about thirty miles an hour, that enables the weather predictions to be made.

Our rapid progress involves the frequent undoing of what has only recently been done in the most costly manner. We have seen expensive buildings erected in the city of New York, and then in two or three years torn down to give way to something greater or different. The Allegheny Portage Railroad, of which my brother, Sylvester Welch, was chief engineer, W. Milnor Roberts being one of his assistants, was considered for some years one of the wonders of the world; the improvements in the locomotive and the increased strength of the rails afterwards enabled engines to cross the Allegheny without the inclined planes used on that road, and that splendid work, on which so much thought had been expended, was torn up. It is folly to build for the far future.

This reminds me that in a paper written in 1829, read before this society two or three years ago, Mr. Moncure Robinson estimated that the tonnage over the Allegheny mountain at that point might in time reach 30,000 tons per annum. I suppose that the tonnage now over the mountain, on the Pennsylvania railroad, exceeds six millions.

One of the bold and remarkable works of the day is the submarine sewer at Boston, to carry the sewage under an arm of the harbor and across an island far to seaward. They have discovered, what unfortunately many others have not, that little is gained by emptying sewage into a harbor or into a small river, and so transferring the nuisance from one point to another, or distributing it all over.

Sanitary engineers have been contending each for his own favorite system of sewering and draining cities. Mr. Hering, in his paper read at the convention at Montreal, impressed upon us that no one system is absolutely good or bad, but either is good when adapted to the circumstances, and bad when it is not. Municipal corporations often think that the remedy for unhealthiness is, of course, sewerage, just as some doctors in old times gave their patients calomel without regard to what was the matter with them, or what kind of constitutions they had.

One of the startling propositions of the day is to bring the waters of Lake George and the upper Hudson by an open canal to supply the city of New York. When somebody asked Brindley what rivers were made for, he said: "To feed navigable canals." The answer now would be: "To supply great cities with water."

Among the subjects to which the attention of the society is now especially turned are Standard Time and the Preservation of Timber. As we expect reports on these, I shall not further refer to them.

One of the most remarkable of modern implements, one whose powers seem almost miraculous, is the diamond drill, which bores into the hardest quartz conglomerate and even into chilled iron. It seems to be capable of much wider application than it has yet had.

* My attention was drawn to this subject by the conference between Espy and Henry, and while traveling in Ireland, I asked my very bright, and on the subjects within his range, intelligent car driver, which way the storms there came from? Evidently he had never thought on that subject, but, adopting on the instant a meteorological creed, answered quick as thought: "The storms, sir, come from whichever way the Lord Almighty chooses to send them."

The attachment of a car to a moving wire rope, in the way proposed by Col. Paine, without injury to the rope or risk to the car, will probably revolutionize the mode of traction in very many cases.

Within the last year or two the load on each wheel of a freight car has been increased from 5,000 lbs. to 8,000 lbs., an increase of 60 per cent. According to Dr. Dudley's observations on the Pennsylvania Railroad, an increase of 60 per cent. on a wheel made an increase in wear per million of tons of a little over 30 per cent. We may expect that this recent increase will increase the wear at least 30 per cent.; that is, the rails on a heavy traffic road that would have lasted with the old machinery 10 years, will now last 7.7 years. But with the heavier weight on a wheel, the residuary part of the rail after it is worn down to the limit of safety, must be much stronger than formerly required, in order to bear the heavier weight. Suppose the diminution of the consumable part of the rail on this account to be 20 per cent. (which would be only 4 or 5 per cent. increase on the whole rail) it reduces the duration to 6.16 years with the same traffic. But as the traffic has increased much more rapidly than was expected, it is now probable that the rails on our heavy traffic roads will not last half as long as they were expected to last three or four years ago. If a rail will last a dozen years where actually used, it would not pay to add more than about thirty per cent. to its cost to make it last two dozen years, but it would pay to add 45 per cent. to its cost to prevent its duration from coming down from a dozen to half a dozen years. Steel rails were made fifteen years ago with twice the endurance of those made now. Under the new circumstances, it is probable that it will before long be economy for roads with the heaviest traffic to pay the railmakers a price that will enable them to make rails as durable as the best ever made.

The concert of action among so many persons, and over so great distances, essential to the safe, efficient and economical operation of our railroads, and, therefore, to the safety and cheap accommodation of the public, makes it necessary that all the operations of a great system should be in one interest and directed by one central authority. These might be governmental, but in our country, at least, experience has shown that this is absolutely inadmissible. It is in the hands of great corporations, who have vast amounts of property and armies of men under their control. In some places every third man you meet wears the button of a corporation. Whether this concentration of power is is itself good or evil, it is inevitable; and certainly a less evil than its alternative. The possession of this power carries with it grave responsibilities, especially in promoting the welfare of their employees.

Many of the best and wisest corporations recognize the duty of regarding their employees not merely as parts of a vast machine, but also as men. Saying nothing now of any higher considerations, they know that if they show a proper interest in their employees, their employees will feel more interest in them; that if they provide a comfortable retreat for their train men when off duty they will not be driven to the liquor saloon for shelter; that if they give facilities for intellectual and moral improvement to the men off duty they will be better, and especially more reliable employees; and that if they give them the day of rest which God and human experience have alike declared to be necessary, they will be more efficient.

Time was when corporations had very limited powers. Now they can do pretty much everything an individual can do, and a great deal besides. So officers could do little without specific authority from the directors. According to my recollection of the minute book of the company, which in 1804 built the celebrated bridge across the Delaware at Trenton, at a cost of $180,000 (a great sum at that time), the very first resolution of the board authorized the president to purchase two shovels and a crowbar.

The subject of uniform time for railroads is now claiming the special attention of this Society. It is of great importance, but it has been so recently and so fully placed before the Society by Mr. Fleming and others that it is only necessary to call attention to their communications.

The subject of tests for large members of metallic structures is now receiving our earnest attention. If I should speak

of its necessity it would only be to repeat what is said in our memorial to Congress. I will only again call attention to one point; that is, that the process of manufacture of a large piece of iron or steel may be so different from that of a small piece, and therefore the quality of the two be so different, though both may be made from the same stock, that the strength of the larger cannot be inferred, but only guessed at, from the known strength of the smaller. In the larger there is more likely to be permanent opposing strains that destroy a large percentage of its strength. A remarkable instance of opposing strains, caused by treatment in manufacture, was pointed out some time ago by Colonel Paine. He found that wire coiled before it was set could not be even straightened without straining the sides beyond the limits of elasticity, and that such wire had nothing near the strength of that coiled stright. As the strength of a large metallic member of a structure cannot be tested by any machine within the reach of individual means, and as to obtain the best results requires the combined skill of several classes of experts, the aid of Congress is invoked to provide a suitable machine, and to create a board of experts whose varied skill shall plan the best experiments.

We are justly proud in this country of the system of checking baggage on our railroads. A traveler gets a check for his trunk at a hotel in Philadelphia, and gives himself no further trouble about it till he finds it at his destination, perhaps in Maine or Texas, or Oregon. It contrasts favorably with the system on the Continent of Europe, and especially with the want of system in England. But our *hadling* of baggage in this country is shocking. A light English trunk will travel all over Europe without injury. Here it is likely to be destroyed in a single trip. The greater weight of the stronger trunks required here costs the railroad companies quite an appreciable amount in the course of a year, and the damage to the trunk and its contents by the rough handling it gets sometimes costs the passenger as much as his fare. And in the great majority of cases careful handling would not cost anything extra.

What is, and is to be, the effect of all the activity and progress of the present day on human welfare?

Doubtless the preponderance of effect is good, but with many drawbacks. I will notice one:

The rapid movement of the business of the world requires an immense amount of brain work to be done by those who direct it in each business day. This is made possible by the recently introduced facilities for rapid work. Formerly, when a man wrote his own letters, he thought sentences only as fast as he could write them. Now he dictates three or four sentences to his stenographer in the time he would have been writing one, and so performs three or four times as much brain work per minute, as he would if the wrote himself. He does not go out of his office to confer with a man at some other office, but sits still and telephones him. When the railroad officer travels on his own road he does not chat with his friends in the public car, but goes in his office car, with his stenographer, clerks and table covered with papers. When a man goes home from his office he does not take the time to walk, but works on till the last moment, then goes on the Elevated Railroad. The brain gets no rest, as it would have got in old times; now constantly rushing forward, not standing in its tracks, as formerly, while the man was writing down the thought of the previous instant; now furiously at work, while formerly resting while the man was going from place to place. This kept up for six or eight hours a day must soon break a man down, and has already broken down some of our ablest men. It does not mend the matter much that next summer he can spend a few weeks at the shore, or among the mountains. A man running up hill till he is out of breath is not enabled to keep on running another hour by the prospect of rest next week. A man that runs a locomotive twenty miles an hour may run all day, but if he runs sixty miles per hour, and so his brain and eye have three times as much to do per hour, he must soon stop to rest.

Undoubtedly the progress of the age, which is so largely engineering progress, does on the whole greatly increase the welfare of mankind. By making the

forces of nature do the hard work, the labors of the toiling millions are lightened many fold. The laboring man now works with brain and eye more than with muscle, and his business is now to apply some principle of science. This raises him intellectually. He now has time for improvement. Comfort and refinement, and even luxury, are brought within his reach. The forces of nature having become obedient to the will of man, they are made to produce for him not only plenty, but conveniences and luxuries formerly undreamt of. By the present facilities the races of men are brought into contact with each other. Those races are being assimilated, and the prejudices and hatreds of the past are fading away. Supreme power among men is more than ever in the hands of the most enlightened, and they are sending civilization and Christianity into the regions most benighted. The light of Heaven is beginning to shine into the Harem and the Zenana. And the time seems to be hastening when there shall universally prevail "peace on earth" and "good will towards men."

WIND PRESSURE.

By WILLIAM FERREL.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

IN the January No. of the ENGINEERING MAGAZINE, p. 49, is an article, copied from *The Architect*, in which is contained a theoretical formula on the pressure of the wind which makes it twice as much as it should be. The importance, often in engineering, of having an estimate of the possible amount of wind pressure, renders it important that we should have correct theories and formulæ upon the subject. Let

U, V = linear co-ordinates respectively perpendicular to, and parallel in any direction with, the earth's surface;

u, v = the corresponding velocities respectively in these directions;

k = the density of the air;

g = the acceleration of gravity.

We then have the well-known equations

$$\left.\begin{aligned} -g-\frac{d\mathrm{P}}{k\,d\mathrm{U}}&=\frac{d\,d\mathrm{U}}{dt^2}\\ -\frac{d\mathrm{P}}{k\,d\mathrm{V}}&=\frac{d\,d\mathrm{V}}{dt^2}\end{aligned}\right\} \quad . \quad . \quad . \quad (1).$$

For our purpose it is only necessary to solve these equations in the special and simple case of horizontal motions, in which case we can assume k to be a constant, and $u=o$. From the last of these equations we get in this case

$$-\frac{d\,\mathrm{P}}{k}=\frac{d\,\mathrm{V}}{dt}\cdot\frac{d\,d\,\mathrm{V}}{dt}=v\,dv$$

and by integration,

$$\mathrm{P}_o-\mathrm{P}=\tfrac{1}{2}\,k(v^2-v^2{}_o) \quad . \quad . \quad . \quad (2).$$

in which P_o is the value of P where $v=v_o$.

With $u=o$, the second member of the first of (1) vanishes and we have by integration

$$gk=\frac{\mathrm{P}}{\mathrm{U}}=w \quad . \quad . \quad . \quad . \quad (3).$$

in which, since we have assumed that k is constant, U is the height of a homogeneous atmosphere of the pressure P, and hence

w = the weight of a unit of volume of air of tension P.

From (2) and (3) we therefore get

$$\mathrm{P}_o-\mathrm{P}=\frac{v^2-v^2{}_o}{2g}w \quad . \quad . \quad . \quad (4).$$

Where the wind is stopped by a perpendicular barrier we have $v_o=o$, and then have, putting $p=\mathrm{P}_o-\mathrm{P}$

$$p=\frac{v^2}{2g}w \quad . \quad . \quad . \quad . \quad (5).$$

In this expression p is the increase of pressure at the surface of the barrier over the general pressure P, and hence it is pressure of the wind, and vanishes when

v vanishes. This expression makes the value of p only half as much as the formula referred to above.

The weight of a cubic foot of water at temperature of 4° C, which is the temperature of maximum density, is 62.431 pounds avoirdupoise, and the density of dry pure air at sea level, on the parallel of 45°, under a barometric pressure of 760^{mm}, and having a temperature of 0° C, according to Regnault, is .00129278. And according to the laws of Marreotte and Gay-Lussac, the weight of a given volume of air is as the pressure and inversely as the absolute temperature. Hence we have

$$w=62.431.\frac{.00129278}{1+.003665t}\cdot\frac{P}{P'}$$

$$=\frac{.08072}{1+.003665t}\cdot\frac{P}{P'}$$

in which $P'=760^{mm}$ P is the barometric pressure of the air under consideration, and t is the temperature according to the French scale. With this value of w (5) becomes, putting $g=32.17$ feet

$$p=\frac{.001255}{1+.003665t}\frac{P}{P'}v^2 \quad . \quad . \quad . \quad (6).$$

in which p is the pressure on a square foot, in pounds avoirdupois, and v is the velocity of the wind in feet per second.

At or near sea level, P : P′ can be assumed, generally, to be equal unity without much error. At the top of Pike's Peak or Mont Blanc it would be about one-half of unity, and hence at these altitudes the pressure of the wind for the same velocity is reduced about one-half. It is seen that an increase of tem perature also decreases the pressure of the wind, but this, in ordinary variations of temperature, does not amount to very much, so that if the numerical coefficient is adapted to some average temperature, the temperature may be neglected with out much error.

Where v is expressed in miles per hour the formula becomes

$$p=\frac{.002700}{1+.003665t}\frac{P}{P'}v^2 \quad . \quad . \quad . \quad (7).$$

The following is Smeaton and Rouse's empirical formula, which is usually found in text books and manuals,

$$p=.00492v^2 \quad . \quad . \quad . \quad (8).$$

For what barometric pressure and temperature is not stated.

Hagen's empirical formula, determined from very accurate experiments only a few years ago, is

$$p=(0.00707+.0001125u)Fv^2 \quad . \quad . \quad . \quad (9).$$

in which p is expressed in grams, u is the periphery of the plate and F the surface of the plate in decimeters, and v is the velocity per second in decimeters. The barometric pressure in the experiments was 758^{mm} and the temperature 15° C.

This formula, with p expressed in pounds avoirdupois, u and F in feet, and v in miles per hour, becomes, when expressed so as to include variations of pressure and temperature,

$$p=(0.003064+.0001191u)\frac{P}{P'}\frac{Fv^2}{1+.003665t}$$

$$\quad . \quad . \quad . \quad (10).$$

It is seen that this empirical formula, in all cases, gives a pressure considerably greater than the theoretical formula (7), but much less than that of (8), unless we suppose the periphery of the plate u to be large. Hagen's experiments were made with small plates varying from two to six inches square. How nearly the formula would hold for much larger plates, remains to be determined from experiment.

The value of p, given by the theoretical formula (7), is the true increase of pressure on the side of the plate exposed perpendicularly to the direction of the wind, and would be the effective pressure of the wind in overcoming obstacles opposed to it, if the pressure were not diminished on the side opposite to that exposed to the wind.

The air, in passing around any barrier, diminishes the pressure on the opposite side, mostly by dragging the air away in passing, through the effect of friction between different strata or portions having different velocities. This is seen in the effect of hoods placed on the tops of flues of chimneys to prevent their smoking. The air is dragged away and the pressure diminished so that the air escapes from the flue more readily.

If we put

p' = the diminution of pressure on the opposite side of the barrier,

we shall then have $p+p'$ for the effective pressure of the wind, such as is obtained by experiment. Deducting (7) from (10), putting F=1, we shall have, according to Hagen's experiments,

$$p'=\left(\frac{.000364+.0001191u}{1+.003665t}\right)\frac{P}{P'}v^2 \quad . \quad . \quad (11).$$

as the effect on the opposite side due to the dragging effect of the wind. It is by the amount of this term that the empirical formula should differ from the theoretical. It is seen that this part increases by Hagen's formula, with the increase of the periphery of the plate, and hence with the size of the plate used in experiments, and with only a moderate increase in the size of the plates, this part of the effective pressure would exceed the other part, and in case of a large barrier, as the side of a house, it would be very much greater than the other. But from experiments made through so small a range, we cannot infer that this would be the case, and we are left very much in doubt as to what it would be, except for the small range for which experiments have been made, but we at least know that in all cases the value of p' cannot vanish, and that the effective pressure of the wind must be considerably greater than the theoretical pressure given by (7).

In Gehler's Physicalisches Worterbuch, Vol. X., Part II., p. 2076, we find the following ratios between the theoretical and experimental wind pressures: Marriotte, 1 to 1.73; De Borda, 1 to 1.66; Bouse, 1 to 1.90; Hutton, 1 to 1.243; Woltman, 1 to 1.19; of these, it is stated, the last one is considered the most reliable, and those of Rouse and Marriotte the least. Rejecting the latter, and giving to Woltman twice as much weight as to Hutton and De Borda, we get the ratio 1 to 1.32. The ratio between (7) and (10), putting F=1 in the latter, is for a circular plate of an area equal to one square foot, 1 to 1.256. The differences between the preceding ratios may have arisen from plates differing very much in size, having been used in the different experiments.

Anemometers constructed upon the wind-pressure principle are the most reliable, since they depend only in a small measure upon friction, and the velocity is determined mostly from the observed pressure theoretically, leaving a comparatively small part of the formula, due to friction mostly, to be determined by experiment for the particular plate used in the anemometer, and to be applied to the theoretical formula in the form of (11). Such anemometers are very sensitive to very small changes in velocity with short periods, such as those which occur when the wind blows in gusts, and observations made with such anemometers are more useful to engineers than those made with Robinson's anemometer, which leaves no record of the maximum velocities of sudden gusts of wind, which usually do the principal damage.

Since by (7) pressures are as the squares of the velocities, it is seen that small changes in velocity produce a much greater change in the pressure, when the regular velocity is great than when it is small. With a wind blowing at the rate of 50 miles per hour (7) gives a pressure of 6.75 pounds on the square foot, but with a velocity of 100 miles per hour it gives four times as much, or 27 pounds to the square foot.

The cause of the winds blowing in blasts in a cyclone, is the small tornadoes which are constantly being formed within it. On the side of the tornado in which the motion around its center coincides with the direction of the wind in the cyclone, the velocity of the resultant wind is the sum of the two, but on the other side it is the difference of the two. Hence when a tornado within a cyclone passes over any place, there is a certain sudden increase of velocity or gust of wind, or there is a momentary lull, according as the one or the other side passes over the place. If the central part passes over the place there is not much change of velocity, but a great change in the direction of the wind causing a sudden oscillation in the wind-vane. Small tornadoes or whirlwinds are being continually formed within cyclones, because the conditions are then favorable for their formation, the air then being generally in a state of unstable equilibrium and having a gyratory motion.

If we express p in (7) in terms of the height of the mercurial column in the barometer, instead of pounds per square foot, it will give the changes of the bar-

ometer due to the wind. The atmosphere under a barometric pressure of 30 inches has a pressure upon the earth's surface of 2116 pounds upon a square foot. Hence, putting

b=the barometric pressure corresponding to p, we have

$$b=\frac{.0027\times 30v^2}{2116(1+.003665t)}\cdot\frac{P}{P'}$$

$$=\frac{.00003827\,v^2}{1+.003665t}\cdot\frac{P}{P'} \quad . . \quad (12).$$

According to this formula, if the wind blows perpendicularly against a wall or any kind of barrier, with a velocity of 50 miles per hour at sea level and temperature 0° C, we shall have b=0.0957, or nearly one-tenth of an inch as the effect upon the barometer placed close to the wall where v=o. Hence, when the wind blows by blasts a barometer so placed is subject to numerous small oscillations, called "pumping." This also occurs when it is placed in a room into which the wind blows, or presses through some open door or window, and has no free egress on the opposite side. There is also some of this observed when the barometer is placed on the opposite side of a barrier, or in a room in which there is a door or window on the lee side. The effect is then produced, not by the changes of pressure due to change of velocity given by (7), but to the smaller effects depending upon changes in the value of p' in (11).

WASHINGTON, June 20, 1882.

THE ANALYSIS OF POTABLE WATER, WITH SPECIAL REFERENCE TO THE DETERMINATION OF THE PREVIOUS SEWAGE CONTAMINATION.

By CHARLES WATSON FOLKARD, Associate Royal School of Mines.

From Proceedings of the Institution of Civil Engineers.

I.

As far as the examination of mineral substances is concerned, analytical chemistry is in a very advanced state. Indeed, it may be a matter of opinion as to whether any improvement is required for practical purposes. But as regards organic chemistry, especially that branch which deals with the secretions and tissues of plants and animals, the reverse is the case, and analysts are at present groping in the dark. Nor is this to be wondered at, when the enormous number, great complexity of composition, and unstable nature of these bodies are taken into account, and also the short time that has elapsed since they were first studied. It is a comparatively simple matter to estimate the percentages of the constituents of a body, in other words to make an ultimate analysis of it; and where one element forms but a few combinations with another, the relative amounts of the constituents determine which of the compounds is under investigation. But inasmuch as hundreds of organic compounds are made up of the same three or four elements, and in many even the proportions of these elements are nearly the same, it is obvious that ultimate analysis will not afford sufficient information to allow of the presence or absence of a certain substance being predicated. If the analyst receive the substance in a pure state, or if it be capable of purification by crystallization, distillation, &c., its physical properties of specific gravity, form, color, &c., are of great assistance in ascertaining its identity. But if a solution in water is the form in which it is received, and especially if the solution be very dilute, the difficulties are greatly increased. When, in addition, the substance itself is very prone to decomposition, and is mixed with other bodies equally unstable and equally hard to detect, a degree of complexity is introduced into the investigation which makes it an almost hopeless task in the present state of chemical science.

Such are the perplexities under which the Water analyst labors, and their care-

ful consideration may serve to account for the wide differences of opinion on this important subject. It is much to be regretted that this uncertainty should exist, and it can only be hoped that in a short time a bright light (possibly by the aid of electricity) will illumine this almost untrodden ground of research.

The author proposes to divide the subject as follows:

1. The various ways in which water becomes contaminated.

2. The methods employed by analysts to detect and determine the extent of this contamination, with an opinion as to the probable value of the results obtained by the various methods.

3. The bearing of the results of biological and microscopical research on the question.

4. The adequacy or inadequacy of the proposed remedial measures, irrigation, chemical treatment, and filtration.

1. The various ways in which water becomes contaminated.

Immediately on the condensation and precipitation of the aqueous vapor of the atmosphere as rain, the liquid dissolves more or less of every substance with which it comes in contact. Oxygen, nitrogen, carbonic acid, ammonia, and nitric acid can be detected, and these may be taken as normal constituents of rain falling on the surface of the earth or on the catchment reservoir of a town. It will also be always more or less contaminated with the excreta of animals, although reservoir water will contract but an inappreciable amount of impurity from this source.

The next stage for consideration is rain water in the form of springs. In addition to the above-mentioned bodies, spring water contains various mineral substances dissolved from the strata through or over which it has passed, the majority if not the whole of which are innocuous in the quantities in which they exist in most specimens; together with a further amount of animal contamination, varying in nature and quantity with the character of the area, as to population and agriculture, in which the springs occur. In remote country districts the contamination of the water up to this point is very slight.

In the next stage, the rivers, there is an enormous increase of contamination. Nor is this to be wondered at, considering that rivers are the natural drains of the country, into which every particle of rain falling within their watersheds (except that evaporated from the surface) ultimately finds its way, with everything which it is capable of dissolving or suspending. Highly manured arable land, pastures with their thousands of cattle and sheep, mills, factories, village cesspools, and, lastly, town sewers, all contribute their quota of foul water; in some cases to such an extent that the river becomes an open sewer in which no fish can live, and the exhalations from which, especially in hot climates, spread fever and death around.

The remaining sources of water to be considered are wells. In country places these may be uncontaminated, but in most cases it is far otherwise, owing to the utter want of foresight in the sanitary arrangements, the cesspool being frequently close to (and of course above the level of the water in) the well. With regard to wells in towns provided with a deep sewerage system, they are generally dry, fortunately for their owners; on the other hand, if the town be provided only with cesspools, the ground is so saturated with sewage matter from the latter that the water is totally unfit for use.

2. Having thus considered the various sources of water supply, and the nature and amount of contamination to which each is liable, the second division of the subject follows—"the methods employed by analysts to detect and determine the extent of the contamination."

The mineral constituents may at once be dismissed, as their determination is a very simple matter; and unless they exist in enormous excess, without doubt they are practically harmless. The organic substances in solution and suspension are the most important, on acconnt of their dangerous nature, and, unfortunately, they are the ones with which the chemist is least able to deal. As yet he has been compelled to be content with the examination and estimation of the products of their decomposition—ammonia and nitrous of nitric acids—or with the determination of one or two of their constitutional elements (carbon and nitrogen). Urine *per se* is by no means a difficult substance to detect and analize; but the examination of water con-

taining one-hundredth or one-thousandth part of urine, a week or two old, is a very different matter. So also with the solid excreta of animals on the one hand, and the same suspended in minute quantities in water on the other. In the present state of analytical chemistry it is impossible to detect either the one or the other in those highly diluted forms. Common salt is abundant in urine, but so it is in many soils, and therefore is generally found in water; and as it is impossible to distinguish between that derived from the land and the same substance contained in sewage, the fact of its presence or absence in a sample of water is not of much importance.

Then, again, rain contains ammonia and nitric acid (if not also nitrous acid), and it becomes impracticable to detect whether these substances, when found in water, are derived from the decomposition of organic matter with which the water has been contaminated, or have simply been dissolved from the atmosphere by the rain in falling.

(*a*) The oldest process for the investigation of the organic matter in potable water is by the incineration of the solid mass left on evaporation of the sample, and it has the great advantage of simplicity. A measured quantity having been evaporated to dryness, the residual solid matter is weighed and heated, finally to bright redness. The evaporation is usually conducted in a platinum dish in a water-bath, by which means loss by ebullition is avoided. The residue, after weighing, is heated to redness in the dish over a Bunsen flame. By this process the organic matter is burnt away, carbonic acid, nitrogen, &c., being given off. At the same time any carbonate of lime or magnesia is decomposed, the carbonic acid being expelled. To correct the error thus introduced, the ignited mass is moistened with a solution of carbonate of ammonia, by which means the quick-lime left again takes up carbonic acid equal in amount to that expelled. It was generally assumed that the magnesia did the same, but this is found not to be the case. The excess of carbonate of ammonia having been driven off by a gentle heat, the dish, with its contents, is again weighed, and the difference, amounting usually to from 2 to 6 grains per gallon, was assumed to represent the quantity of organic matter present. Unfortunately, many water residues show a gain of weight by this treatment, and it has been conclusively proved that it is impossible to measure the quantity of organic matter by this method; but as it affords useful hints as to its nature, it cannot well be dispensed with. For instance, if, on heating, the dry residue blackens, and an offensive smell (especially one of burnt hair) is given off, the existence of nitrogenous animal substances in the water is conclusive, and in nine cases out of ten these substances are animal excreta of recent origin. If, on the other hand, there be little or no liberation of carbon (and consequent blackening when the water residue is heated), and if sparks be noticed, or the peculiar smell of burning touch-paper be perceived, organic matter and nitrates or nitrites are indicated, by the mutual reactions of which, at high temperatures, these effects are produced. From this it can be inferred that part of the organic matter has been oxidized and converted into the harmless salts of nitric or nitrous acid, while another portion remains undestroyed in the water.

Again, if the blackening produced by ignition speedily disappear by contact with the air, the organic substance from which the carbon was liberated was most probably of vegetable origin, and therefore less dangerous to the animal economy. If, on the other hand, the carbon burns off very slowly, it was probably derived from animal substances, which are the most objectionable forms of organic impurities.

It will be as well to point out at once, however, that there is a fundamental objection to the process in the very fact of the evaporation of the water. There is no evidence to show that such unstable bodies are not partially, or in some cases totally, destroyed during the process. Indeed, with one of them (urea) this is known to be the case.

(*b*) The process introduced by Drs. Frankland and Armstrong is open to the same objection, a prolonged evaporation of the water, and although this is effected at a temperature below the boiling point, it is complicated, and in all probability rendered far more destructive to the organic matter which it has been devised to estimate, by the presence of mineral

acids during the evaporation. The residual solid matter is submitted to ultimate organic analysis, by which the amount of nitrogen and carbon is computed. The process is as follows: The water residue is intimately mixed with oxide of copper, and transferred to a tube, ½-inch in diameter and 12 or 15 inches long, which is then completely exhausted of air by a Sprengel pump. The tube, with its contents, is heated to bright redness, till no more gas is evolved, and the products of the reaction (consisting of steam, nitrogen, and carbonic acid) are pumped out into a tube full of mercury standing in a pneumatic trough. The steam is condensed, but the nitrogen and carbonic acid are separated and measured, and from the number of cubic inches of each gas obtained, the weights of nitrogen and of carbonic acid (and from that, of the carbon itself) are easily deduced. At a red heat, oxide of copper decomposes all organic substances, animal or vegetable, transforming their carbon into carbonic acid gas, and their hydrogen into aqueous vapor, while the nitrogen is liberated in the free state, also as gas. The presence of mineral acid during the evaporation is necessary to drive off the carbonic acid, usually a carbonate of lime or magnesia, which, if it were not previously got rid of, would be expelled by the red heat and mix with the carbonic acid formed from the organic matter, so causing an error. The nitrogen and carbonic acid collected are measured over mercury; the carbonic acid is then absorbed by a solution of potash, and the gas left, which is nitrogen, is measured, the difference being the carbonic acid.

Having thus obtained the weights of carbon and of nitrogen existing as organic matter in a certain volume of the water, or rather that portion of the organic matter which has not been decomposed by the prolonged heating with mineral acid, the quality of the sample is inferred from their amount, and from the ratio which they bear to one another, it being assumed that the greater the ratio of nitrogen to carbon, the more highly organized, and therefore the more dangerous, is the organic impurity. A very little thought, however, will suffice to show that the information thus obtained is only of the most general character. Assuming, then, that a high ratio of nitrogen to carbon is characteristic of the organic matter in a dangerously polluted water, if a further pollution by organic substances, in which the nitrogen-carbon ratio is small, take place, the doubly-fouled water would be returned as the less dangerous. This example shows the weak point of the process, or rather of the deductions made from the data furnished by it, namely, the application to a mixture of substances (the organic impurities of water) of reasoning which can, properly speaking, only be applied to the case of a single substance.

(*c*) A process which has found much favor amongst analytical chemists is the so-called albumenoid ammonia method. It is assumed that the nitrogenous organic impurities in water are the most dangerous, which is probably the case, and the process professes to estimate the quantity of these substances, by determining the amount of ammonia produced by their decomposition when boiled with an alkaline solution of permanganate of potash. A glass retort and Liebig's condenser are used, the amount of ammonia formed being estimated in the distillate. This is effected by making up solutions of ammonia of different known strengths, and observing which of them gives a brown coloration of the same intensity as the sample under trial, when mixed with a solution of iodide of mercury and potassium.

No previous evaporation of the water is necessary, which is undoubtedly a great advantage over the first two processes; but inasmuch as this method is only an imperfect ultimate analysis, even less knowledge is obtained than by the second method, though this has the great advantages of ease of manipulation and rapidity, the results being in all probability of equal value for practical purposes.

(*d*) The last to be considered is the permanganate process, in which the amount of permanganate of potash required to oxidize the organic matter is ascertained. This is supposed to be an index of the quantity of organic matter in the water, and it would be so if only one form were present; but inasmuch as there may be dozens of different substances in solution or suspension, some hurtful, some harmless, some susceptible of much oxidation, some almost, or even

totally, unacted upon by permanganate (and so far as is known the most dangerous may consume the least oxygen, or none at all), it is obvious that this method also will not afford results the accuracy and reliability of which are above suspicion.

The estimation of the ammonia, nitric, and nitrous acids in water, is a simple problem in mineral analysis, of which it will be unnecessary to treat in detail.

Having briefly reviewed the advantages and defects of the various processes for estimating the nature and the amount of the organic contaminations of potable water, it seems impossible to come to any other conclusion than that the subject is as yet beyond the scope of analytical chemistry. Even granting that the assumptions of the advocates of the different processes are correct, it is evident that their deductions are illogical, reasoning fit for a single substance only being applied to a mixture of substances.

As regards inorganic analysis the processes can be checked by experimenting on weighed quantities of pure substance purposely mixed with other bodies. If the same amount is recovered (within the small limits of errors of experiment), the process is evidently a reliable one; but with the impurities of water this is impossible, and the information afforded by the methods now in use is of the vaguest and most general character, so far as the wholesomeness or the reverse of a given sample is concerned, although by one of them (*b*) it is possible to determine the minimum amount of contamination which has taken place since the water was precipitated as rain. For this purpose the whole of the nitrogen existing in any form in the water is determined, but this does not include free or gaseous nitrogen dissolved from the atmosphere, which is expelled in the preliminary evaporation, and therefore does not affect the results, viz.:

Nitrogen in the form of ammonia.
" " " organic matter.
" " " nitric and nitrous acid.

Deducting from this total the average amount of nitrogen in the form of ammonia which exists in rain as it falls, the residue is the minimum quantity which the water has acquired from animal and vegetable contamination. It is not necessarily the total quantity acquired, because some may have been abstracted by growing plants, &c.

No definite impression is conveyed to the mind by the statement that there are in a sample of water so many parts per 100,000 of nitrogen, derived from animal and vegetable detritus. A standard of contamination therefore becomes desirable, and the one which has been proposed is the amount of nitrogen per 100,000 parts of average filtered London sewage. By simple proportion it is then easy to calculate the degree of contamination of any water; that is as if 100,000 parts of pure water had been mixed with so many parts of London sewage.

It must be borne in mind, however, that no distinction is made in this case between nitrogen present as organic compounds of more or less dangerous character, and nitrogen existing in the harmless inorganic salts of ammonia, nitrous and nitric acids. This latter form of nitrogen represents more or less originally dangerous organic impurities, which have been gradually resolved by oxidation or fermentation into the inorganic forms. Consequently a deep well-water, *e.g.* from the Chalk, may be returned with perfect accuracy as having received as much or more "previous sewage contamination" than a shallow well or river, and yet in the former case the water may be absolutely innocuous (all its organic impurities having been destroyed by oxidation in the pores of the Chalk), whereas the well or river water, with its recent contamination, may be quite the reverse.

The first stage in the oxidation of nitrogenous organic matter is the production therefrom of ammonia, which by further oxidation is converted into nitrous or nitric acid.

3. Chemists being powerless to help the sanitarian in discriminating between wholesome and unwholesome water, it seems essential to consider what can be done by microscopists and biologists. In the first place it is an ascertained fact, proved beyond the possibility of doubt, that mere dilution, how far soever it be carried, does not render inoperative the specific action of living germs, and so marvelous is the rapidity of reproduction of low forms of life, that if the environment or conditions are favorable to their growth, it matters little whether the liquid is stocked with ten or with ten

thousand at the commencement. In a few days there will be as many as can exist, the only difference being that the sample which received most of the contaminating liquid will arrive at the maximum a few hours before the other. There can be little doubt but that the same thing occurs in the case of the human subject. Provided the individual is sufficiently weakly or unhealthy, it is of small importance whether he receive 1,000 or 1,000,000 parts of infectious matter (whether in the form of organized germs or not is immaterial), and consequently 1 part of infected sewage containing the dejecta of persons suffering from zymotic disease mixed with 1,000,000 parts of water will be nearly as dangerous to him as 1 part per 1,000. Of course the less contaminated water would probably not affect a person in more robust health who might succumb to the use of the highly contaminated sample; but what the author wishes to insist upon is that it will be impossible to banish zymotic disease from a town whose water-supply has been contaminated with the dejecta of patients suffering from that disease. The very weakly will contract it from the almost inappreciable amount of infection contained in the water, and from them it will spread to those who have resisted the poison in its diluted state.

Secondly, the germs which cause or accompany disease are endowed with the most persistent vitality, and are capable of withstanding heat, cold, moisture, drought, and even chemical agents, to a marvelous extent. So difficult is it to destroy them that for many years the now exploded doctrine of spontaneous generation found talented supporters, who relied on their own carefully conducted experiments to prove the theory, all which experiments were subsequently found to have been rendered illusory by the astounding vitality of these low forms of life.

Bearing in mind, then, the influence, or rather the absence of appreciable influence, of mere dilution, and the difficulty with which infectious matter is destroyed, the conclusion that once contaminated water never purifies itself sufficiently to be safe for dietetic purposes becomes inevitable; and as chemical analysis fails to give reliable evidence as to its fitness or the reverse, the author believes that the only safe test of the wholesomeness of a given water is by tracing it to its source, and ascertaining that no objectionable impurities gain access to it.

This will at once condemn all rivers flowing through a populous country; and if it be considered that a river is the natural drain of a district into which everything soluble or suspensible in water ultimately finds its way, it will not be a matter of wonder that this should be the case. No Conservancy Board can keep pollution out of a river; it must receive all the rain falling within the limits of its watershed (excepting, of course, that which is evaporated), together with the overflowings of cesspools and the sewage of towns within the same area. It is part of the great circulatory system of the earth which it is vain for man to attempt to control.

This being so, it is evident that rivers, except near their source, can only afford polluted water, and a problem utterly insoluble by man is presented, viz., the purification of foul water on a large scale. The chemist can do it in the laboratory, but only by adopting a similar process to that by which it is effected in Nature—fixation of the ammonia in the soil or its oxidation to nitric acid, followed by distillation by the heat of the sun. Take, for example, the case of a river with a town of 50,000 inhabitants on its banks. If supplied with water at high pressure and sewered, the amount of foul water discharged into the river will be about 1,000,000 gallons daily, irrespective of the rainfall, which will bring with it the washings of the streets, &c. Taking the total flow of the river at 500,000,000 gallons, and supposing that the water is perfectly pure when it reaches the town, there will be a mixture of 1 part of sewage in 500 parts of clean water, for the inhabitants of the next town to drink. Take now an infected liquid and add 1 part to 500 or even to 500,000 parts of liquid susceptible of infection. The mixture will swarm with low organisms and become putrid in a few days, provided only the conditions are favorable. And what may be expected to happen to the unfortunate inhabitants of the lower town? Simply this, that the strong and healthy will

have sufficient vitality to throw off the poison, but the weak and sickly will succumb, inoculated by the dejecta of zymotic patients in the upper town. Such a state of things seems hardly possible in a civilized community.

The above is no fanciful picture. The experiment was tried on the inhabitants of a town in Surrey, unwittingly it is true, but on that account the result is all the more reliable. An epidemic broke out, and the consequent investigation revealed the cause in all its loathsome details. Fortunately for mankind at large the relation in this case between cause and effect was distinctly traceable, but in the great majority of cases this is out of the question.

There is not the least evidence to show that foul water is rendered wholesome by flowing 50 or 100 miles; indeed, all experiments point in the opposite direction, on account of the persistent vitality of the organisms which accompany zymotic disease, and of the utter failure of dilution to disarm these potent germs of corruption and death.

4. The possibility of abating these evils, otherwise than by a radical change, will now be investigated.

It is often asserted that as the sewage of towns is "treated" by chemical agents before being passed into the river, the previous objections do not hold good. But inasmuch as most of the soluble matters are unaffected by the process, and in view of the great vitality of the low organisms, it is open to doubt if the latter are destroyed by the agents used. Even the irrigation process, the most natural, simple, and effective where the locality is suitable, is liable to the serious objection that part of the sewage may flow direct to the river through accidental channels, without filtration through the soil.

Putting, however, all this aside, those who are practically acquainted with the subject are perfectly aware that no sewerage system yet carried out (even though its cost be reckoned by millions sterling) can cope with storm water. As a necessary consequence the by-pass must be opened, the sewage allowed to flow direct into the stream, and the inhabitants of the town below regaled with a more than ordinarily filthy beverage for the next few days. This again is no fanciful statement; it can be seen in operation more or less frequently all over the country.

Filtration is another remedy put forward as infallible by those who have not grasped the subject. How can filtration affect substances dissolved in water? and as for the minute organisms found in putrescent bodies, they could pass a hundred or a thousand abreast through the interstitial spaces of ordinary sand, as used for this purpose.

In the author's opinion, and probably also in that of most people who have carefully and dispassionately considered the subject, the purification of diluted sewage to a sufficient extent to render it safe for dietetic purposes is an impossibility, putting sentiment aside altogether. Indeed, the mere idea of one community drinking the diluted sewage of another would be almost inconceivable, were it not unfortunately a fact, and one with which the alarming increase of cancerous diseases of the stomach and intestines is in all probability, intimately connected.

The present methods of water analysis are quite capable of showing if contamination has taken place, at all events in the majority of cases; but as to whether that contamination is injurious to health or not, there is no knowledge, and consequently the only safe course in the author's opinion is to reject all sources of supply unless they stand the test of absolute freedom from organic substances so far as can be ascertained; or preferably, of rigid examination by tracing the water from the time it falls to the earth as rain till it enters the reservoir or well.

DISCUSSION.

Mr. Baldwin Latham said he concurred with the author in the conclusion that the chemist was not able to determine whether water was wholesome or not. He used the word "wholesome," whereas the chemist used the word "pure." The purity of the chemist simply meant that he compared water with a given standard, and if it came up to that standard he said it was pure, and if not it was impure. But the impure water of the chemist was not always unwholesome water, nor was the pure water of the chemist always wholesome. He differed from the author, however, in re-

gard to some points, as, for instance, that river exhalations were injurious, spreading fever and death. Mr. Latham maintained, on the contrary, that there was no evidence to show that exhalations from polluted rivers had proved to be detrimental to health. Every authority agreed upon the point that malaria was never extricated from water surfaces, and in malarious countries it was not until the water had disappeared that malaria became manifest. In this country there were sufficient examples to show that the exhalations from foul rivers were not unwholesome. He might instance the case of the year 1858, before the sewage was discharged lower down the Thames, when the foul tide flowed through London. It was a year of drought, and great stench prevailed along the banks of the river, but the mortality tables did not indicate that the districts bordering upon the Thames had in any way suffered. He might quote other towns, like Norwich, where the river Wensum was formerly polluted in a similar way to the Thames, thereby causing a great nuisance to the villages below, yet not one of them had suffered in health from the exhalations. He could not agree with the author that there was no evidence to show that foul water was rendered wholesome by flowing 50 or 100 miles, and that dilute sewage (meaning, he presumed, water contaminated by sewage) could never be made safe for dietetic purposes. Nor could he agree with the statement as to storm-water overflows, but as that was no part of the question under discussion he would not dwell upon it. The subject of the paper was one of considerable importance to those engaged in questions of water-supply, for he regarded the future improvement of the sanitary condition of the country as being almost entirely dependent upon the attention which must be paid to the selection of water-supplies, and the means to be adopted for effecting the purification of water. At present, if engineers were to take the dictum of some chemists, it was quite clear that there was no water-supply fit for use. In the sixth report of the Rivers Pollution Commission it was stated "that it is in vain to look to the atmosphere for a supply of water pure enough for dietetic purposes." Now, as all sources of water-supply were due to atmospheric causes, and the author had stated that it was useless to look for purification by any mode which would be adopted by the engineer, such as filtration or percolation (because the germs, he said, could pass a thousand abreast through a filter), therefore if the rain-water was impure as its source how could it ever be purified? Indeed, if the water-supply of the country were in such a lamentable condition, the wonder was that there was any one living to describe the state of things. The chemist could not discover what were the dangerous impurities in water. In order to supply a deficiency in the paper, or the furnishing of facts to substantiate the proposition put forward, he would read an answer given to a question by Dr. E. Frankland in the Middlesborough water case. Q. 5,052. "And do you think it most unsafe to supply a large population from water which has been impregnated with the excreta of patients suffering from various diseases? I do; although chemical analysis may fail to detect anything unusual in the water, because I have myself mixed 1 volume of the dejection of a patient dying of cholera with 1,000 volumes of good water, and have submitted it to analysis, and have been unable to detect anything unusual in the water; chemical analysis is unable to detect these small quantities of morbific matter, which are calculated to transmit disease to people drinking the water." That was the opinion of one of the most distinguished chemists of the day. With reference to the amount of contamination in water capable of producing disease, he would quote from a little book on "Portable Water," by Mr. Charles Ekin, F.C.S. Mr. Ekin stated, p. 15, "Waters which have undoubtedly given rise to typhoid fever have been found by the writer over and over again not to contain more than 0.05 part of albuminoid ammonia in 1,000,000, and which notwithstanding their containing a large excess of nitrates have been passed by analysts of undoubted ability as being fit for drinking purposes." In an outbreak of typhoid fever at Guilford in 1867, it was clearly shown, on analyzing the water which was the supposed cause of the outbreak, that it was purer than other samples on which no suspicion rested.

In all the calculations of the chemist it appeared to be only a question of degree; they could neither distinguish between the matters which were found in the water, nor the source from which they were derived. If a certain quantity of organic matter, whether sewage or the "germs" of disease, was mixed in the proportion of 1 part to 4 parts of pure water the chemist would call the mixture good water. On the 29th of November, 1875, when an epidemic of typhoid fever was rife in Croydon, there were great suspicions respecting the quality of the water supply. The level of the water in the well at the waterworks was lowered by pumping, and three samples of water were collected as they trickled into the well. They were submitted to Professor Wanklyn, who gave the amount of albuminoid ammonia in the respective samples as 0.14, 0.26, 0.22 per million parts. He stated that two samples were highly charged with sewage and that the other sample was not pure; but in the well the water contained 0.04 of albuminoid ammonia, and he added that that was water of the purest class. Thus, from the examination of the chemist, it appeared that it was quite possible to mix water which the chemist condemned as impure with that which was pure, and the result woald be that the water came out as belonging to the purest class. As to the question of albuminoid ammonia being the means of showing whether water was wholesome or not, he might mention that about the end of the year 1880 the chairman of the Nantwich Local Board of Health told him that the Medical Officer of health of Mid-Cheshire had condemned the public water-supply of the town as totally unfit for domestic use. The supply was taken from a natural lake called "Baddiley Mere," and was brought a distance of 4½ miles by gravitation into the town. The authorities had only power to draw off to a certain depth the top-water. It appeared, from an examination in October, 1880, that the amount of free ammonia was 0.21, and of albuminoid ammonia 0.44 in a million parts in the unfiltered town water, but after efficient filtration the amount of free ammonia was 0.08, and of albuminoid ammonia 0.38. The chemist stated in regard to it, "Organic matter in great excess, rendering water dangerous and unwholesome; the contamination not recent; filtration of little use." In the month of November a second analysis was made, and the results were a little better. The filtered water showed 0.32 part of albuminoid ammonia instead of 0.38, and the remark by the chemist was "the least said about these the better." The report also contained the analyses of the well-waters in use in the town, which were, without exception, very unsatisfactory from the chemist's point of view. He then inquired of the Chairman of the Local Board what was the state of health in the town; he was informed that it was never better, and he therefore advised the Chairman of the Board that as long as the public health was so satisfactory to pay no attention to the alarming reports of the chemist. The Registrar-General had since issued four quarterly reports on the health of the district, namely, for the fourth quarter of 1880 (embracing the period in question), and three quarters in 1881. During the year there had been one death from scarlet fever, two from diarrhœa, and one from fever, the population of the district at the census of 1881 being 11,192. The zymotic death rate in the year was but 0.35 per thousand, or about one-tenth the zymotic death rate of London in the same period, and was one of the lowest that it was possible to conceive in any district, and yet the district was supplied with "dangerous and unwholesome" water.

The following table showed the relative amount of average impurity in the water supplies of London, as ascertained by Dr. Frankland, together with the death rates in each year. The investigation was begun in 1868, when the impurities in the Thames were called 1,000 parts. With that number the relative amount of impurity in other years and other sources of water supply was compared. The numbers were proportional.

The highest annual death rate, and the highest zymotic death rate in London (1871) occurred when the impurities in the Thames and Lee were below the average, and the waters of the deep wells were freest from impurities. The high fever death rate in 1868 occurred when the impurities in all the sources of water supply were below the average. The lowest death rate in London occurred in 1872,

Year.	Proportion of organic impurity in Thames water delivered in London.	Proportion of organic impurity present in Lee water as delivered in London.	Proportion of organic impurity in deep well water as delivered in London.	Annual death rate of London per 1,000.	Death rate of London from seven principal zymotic diseases per 1,000.	Death rate of London from fever per 1,000.
1868	1,000	484	254	23.5	4.82	0.78
1869	1,016	618	312	24.6	5.57	0.78
1870	795	550	246	24.1	5.19	0.63
1871	928	604	150	24.7	5.97	0.54
1872	1,243	819	221	21.4	3.84	0.41
1873	917	693	250	22.4	3.32	0.45
1874	933	583	287	22.4	3.29	0.46
1875	1,030	751	250	23.5	3.87	0.37
1876	903	562	246	22.0	3.56	0.33
1877	907	596	243	21.5	3.43	0.35
1878	1,056	747	323	23.0	4.05	0.37
1879	1,175	954	387	22.7	3.25	0.29
1880	1,263	1,143	393	21.6	3.64	0.24
Averages.	1,013	708	273	22.9	4.14	0.46

when the impurities in the Thames and Lee were above the average; and in 1880, when the death rate was low, all the sources of water supply contained impurities in excess. The zymotic death rate of London was lowest in 1879, when all the sources of water supply contained impurities above the average; and under similar circumstances the fever death rate in London was lowest in 1880. In the year 1870 the waters of the Thames and Lee contained the least amount of impurity in all sources of water supply, yet during the same period the death rate had steadily declined. He did not wish to impugn the character of the chemists; they were men of great honesty and ability, and they themselves confessed the things to which he had referred. Dr. Frankland had admitted that small quantities of morbific matter could not be detected by chemical analysis. But there was a vast amount of ignorance among the general public on the subject, and he had himself to combat it to a great extent in the case of investigations made at Croydon. Dr. M. F. Anderson, in a letter to the *Sanitary Record* of February 3d, 1877, stated, with regard to the albuminoid ammonia process, that he had "never been able to obtain conclusive evidence that the dangerous elements of bad water are evolved as albuminoid ammonia;" and he added, "My observations tend rather to the belief that typhoid germs are easily oxidized, and do not yield up their nitrogen as ammonia, but as nitro-oxides." That rather went back to the question of previous sewage contamination, which seemed to be almost a phantom of the past, as it appeared to have been abandoned by its author; but he thought there was something in it, because it certainly showed the progressive impurities that took place in water. From the report of the Royal Commission on Water Supply, it was shown that in the district from Caterham to Croydon there was a very considrable increase in the previous sewage contamination; or a progressive degree of deterioration in the water had taken place. Those who were conversant with the district would know that there must have been such deterioration because the valley was thickly populated; it had two waterworks in its upper part; it had no sewers whatever; all the water pumped passed through cesspools, and by a sort of circulating system all the impurity was carried back into the soil, and which flowed down the valley, and what was not used naturally found its outlet in the river Wandle. It was evident that in a valley of that kind there must be a natural deterioration; but unfortunately the chemists had never been able to find it, for although the previous sewage contamination had enormously increased, that counted for nothing with the chemist at the present day. In such a district, however, what might have been proved to be serious sewage contamination was very likely to become present sewage contamination of the most dangerous description. In the epidemic of fever in Croydon in 1875 the water had been analyzed over and over again; but it was always pronounced to be water of the purest class; yet in that year one person in forty-two living in the Croydon water district suffered from typhoid fever as against one in eight hundred and nine in the district immediately outside, and in many instances the same sewers were used in common. Numerous investigations had taken place in connection with the subject, and he had himself inquired into it, feeling that it was an utter disgrace to the sanitary science of the day that those repeated epidemics in Croydon should escape detection. They had always been referred to the same cause—

sewer gas; but he believed that he should be able, from the facts he had collected, to throw a very different light upon the subject. If repeated coincidences were tantamount to positive proof, he believed he should be able to show that certain meteorological conditions were connected with the outbreak of every one of those epidemics, which came into operation only at particular times. One thing was certain, that at all times the fever death-rate in Croydon was inversely proportionate to the quantity of water flowing from the district. The author had stated that it was necessary to trace water to its source. But that had been the difficulty in Croydon. The late Dr. Letheby, who analyzed the Croydon water, found it to be good; but that did not satisfy his mind, for he distinctly reported to the authorities of the Friends' school, by whom he had been called in, that the water-supply was dangerous by reason of its source in the center of the town. Mr. Latham at one period held the same views as Dr. Buchanan, who reported on this outbreak in 1876, that fever was caused by sewer gas; but he had seen reason to alter his opinion. The difficulty, however, had been to trace the water; but during the past year, not only had the movement of the subsoil water been traced, thanks to the ability of a chemist in the city, but Mr. Latham had been able to bring the matter under direct calculation, and to show the quantity of the immediate subsoil water getting into the Croydon wells. The case was this. The wells furnishing the supply of water to the town had been sunk and bored into the porous soil, consisting of gravel and chalk. They were lined with iron cylinders for a certain distance from the surface, and the subsoil water outside the wells was supposed to be shut out by the iron lining; yet when pumping went on every fluctuation within the wells was discernible in the subsoil water outside. It had been stated by an eminent engineer that these fluctuations simply meant that there was a sympathy between the waters. Other theories had been advanced, one of which might be called the "band-box" theory. It was stated that when the water outside the well subsided, it did not flow into the well, but that it was like a tier of band-boxes, the bottom one might be pulled out, but the top one would not come down. Then it had been referred to pulsations, or waves caused by the agitation of pumping. Fortunately, for the sake of science, on the occurrence of a bourne flow at Croydon, early in 1881, he received a communication from Mr. G. W. Wigner, that if Mr. Latham would collect the samples of water during the bourne-flow he would be happy to investigate the matter from a chemical point of view. After the collection of the samples, Mr. Wigner wrote to him that it would be desirable, as the next step, to trace the movement of the underground water by means of lithium. He saw at once that this was exactly what was required to ascertain whether or not there was a connection between the immediate subsoil-water outside the wells and the water within the wells, and if the fluctuations which had been observed were indicative of this connection. Before making any experiments, however, he put two questions to Mr. Wigner, one of which was whether the material was innocuous, to which the reply was, "perfectly innocuous," and the other whether small quantities of the material could be detected, to which Mr. Wigner replied, "Yes, $\frac{1}{3000000}$ part of a grain can be found in a gallon of water by spectrum analysis, but in no other way." Three experiments were made at various distances from the Croydon Water Works wells, and it had been shown that the lithia moved in all directions, exactly at the same rate, into the wells, as the fluctuations in the water caused by pumping had been found to move. Lithia afforded, therefore, a mode of readily detecting the movement of water. It was admitted that the subsoil-water at Croydon was in direct communication with the sewers, and if it got into the wells, it was a source of danger. There were great difficulties in carrying out the investigation, because the lithia could only be detected by spectrum analysis. Again, when material of that kind was put into the soil, a portion of it remained and was with difficulty got rid of, for when an acid salt had been put into a chalk soil, a portion of the acid combined with the chalk, and a less soluble salt of lithia remained in the soil. Investigations of this kind should only be carried out under the advice, and with the assistance

of a chemist. He did not think that Nature had left mankind in the unguarded and unprotected state described by the author, liable at any moment to have their lives jeopardized from impurities in water. There were means, no doubt, by which the very foulest water could be purified, and those means were more active in a river than in any other source of water supply. He would refer to the statement of Mr. T. Hawksley, Past-President Inst. C.E., with reference to the outbreak of cholera in 1848–9, recorded in the report of the Commissioners of Water Supply, that in those years cholera was epidemic at Bilston, Wolverhampton, or in the Black Country; and so violent was it that people encamped outside the towns. During the whole of that time the sewage of those infected places flowed into the Tame, and, after a course of 20 miles down the river, it was used for the water supply of Birmingham, and there was no cholera in Birmingham. It was therefore clearly shown that by the simple flow of the water that distance the morbific elements had been destroyed. He might also refer to a more recent period, 1875–76, when typhoid fever was prevalent in Croydon, there being at least two thousand cases in those two years, during which time the whole of the sewage of the town was passed on to the farm at Beddington. There was a cluster of eighty houses lying between the farm and the Wandle, all inhabited, their only water supply being from shallow wells, and the proximity of the application of the sewage upon the farm caused the water in these wells to fluctuate, yet the elements of disease were destroyed so that there was not a single case of typhoid in any one of those houses, or even in the valley down to Merton, containing a considerable number of inhabitants. There again it was shown that Nature had provided safeguards; and it was the duty of engineers to copy the examples of Nature, and to treat water in the way in which Nature treated it, in order that the foulest and most dangerous impurities might be destroyed or removed from it.

ON THE PROTECTION OF BUILDINGS FROM LIGHTNING.

By CAPTAIN J. T. BUCKNILL, R.E.

From the "Journal of the Royal United Service Institution."

A FEW weeks ago, when I accepted the invitation of the Council of this Institution to read a paper on the application of lightning conductors to buildings and magazines, it never occurred to me how difficult would be the task to deliver an *interesting* paper on so special a subject, or a paper that would be of value to a purely naval and military institution. It is, however, only too true that lightning strikes soldiers, sailors, and civilian alike, and that the laws which should govern the application of conductors are the same whether it be a palace or a jail, a chimney, a cathedral, or a man-of-war that has to be protected. Moreover, the immense interests jeopardized by any faulty arrangements, which might occasion the explosion of magazines, makes the subject of special importance to naval and military men. Imagine the loss to the war strength of the Empire which would be entailed by the accidental explosion of one of the large magazines at Tipner or at Priddy's Hard, with its charge of, say, 750 tons of gunpowder, or over 750 millions of foot tons of energy developed in less than one second of time, and this within a short distance of the greatest naval arsenal in the world, and a town with 120,000 inhabitants. Every building shed would be leveled to the ground, and the town would be visited as was Chios the other day. The proper application of lightning conductors to large magazines and to men-of-war is evidently therefore a matter of importance to us all.

Electricity exists in two distinct forms, the static and dynamic, but the word static thus applied is somewhat misleading, because electricity (like heat) is now recognized to be a form of matter in motion, whether in the state of

potentiality as in a thunder cloud, or in the state of activity (the work-producing state) as in lightning.

How the former is produced is still conjectural, although a multitude of theories have been propounded.

In whatever manner the electricity is produced, the thunder clouds act as collectors; and more than this, when the surface of the earth beneath them is not far distant, and is composed of fairly good conducting media, the earth, the clouds, and the intervening air form huge condensors—the electrified clouds acting by induction upon the earth, and the latter reacting upon the cloud.

Now the amount of electricity of given potential which a cloud is capable of receiving depends firstly upon its size, the amount varying directly as the linear dimensions of the cloud; and, secondly, upon the intensity of inductive action of the earth's surface, the cloud's power of receiving electricity being greatly increased thereby.

For example, a cloud of given dimensions at an altitude of 300 feet could be charged by 80 times the electricity that would charge it were its altitude increased to four sea miles.

For a similar reason a cloud over a conducting area could be charged much more highly than the same cloud at the same height over a non-conducting area.

One of the most remarkable of the phenomena connected with electricity is the mutual attraction of bodies charged with electricity of opposite sign, and the mutual repulsion of bodies charged with electricity of like sign. Now the charges on inducing and induced surfaces are always of opposite sign. The bodies possessing these surfaces consequently attract each other. If, therefore, thunder clouds be driven by the wind or otherwise over portions of the earth's surface which vary considerably in their conducting power, they will be attracted to those regions which from their conductivity present the greatest facilities for inductive action; and this, in spite of the mutual repulsion of the clouds; just as the numerous admirers of a beautiful woman, although hating each other, are attached to her.

Now it generally happens that the thunder clouds in a storm are sufficiently numerous to cover both favorable and unfavorable areas of the earth's surface, and, as little or no inductive action occurs over the latter, but very considerable action over the former, the electrostatic capacities of the clouds become greatly altered, and lightning plays from cloud to cloud, until those which are situated over the earth's conducting surfaces become so highly charged that the electricities are able to overcome the resistance of the intervening air and to unite across it by what is termed the disruptive discharge. This is lightning.

I have been thus particular is describing the action produced by the earth's surface upon thunder clouds, because the somewhat important conclusion must be arrived at, that lightning is most to be feared by those who live on well-conducting areas, even of low elevation; and that lightning is least to be feared by those who live on non-conducting areas. This is shown on plate, Fig. 9. where the distribution of the electrical charge is shaded in. The cloud over the Portsdown Hill, although nearer to the ground, is much less highly charged than the cloud over Portsmouth and Spithead, because the former presents a non-conducting area. This electrical distribution is of considerable importance, and it shows that it is much more necessary to provide lightning conductors for buildings situated upon a damp clay or boggy bottom than for those on a chalk down. This is very convenient, for it is almost impossible to make an efficient earth connection in the latter situation.

As before stated, disruptive discharge constitutes a lightning flash. Immediately before the stroke the particles of air are subjected to a high strain by static induction, producing a polar tension which is proportional to the square of the potential. Faraday's experiments proved this, as well as the fact that the stroke tends to traverse the air in the direction of such polarity. The tendency of lightning is therefore to strike in a direction normal to the earth's surface.

But there is another mode by which thunder clouds are discharged, viz., by the brush discharge.

Electricity of high potential leaks, as it were, from conductors which are provided with projections in the nature of points, where the distribution of electri-

cal density is greatest, a stream of electrified air being thrown from each point, and the charged conductor robbed by continuous streams of its electricity in this manner.

Although the brush discharge is frequently so intense as to be luminous to a height of 6 or 8 inches, it is not attended with any appreciable heat. Its action should therefore be fostered, as it often wards off a dangerous stroke of lightning by neutralizing the opposing electricities in a harmless manner.

It has been observed so late ago as 1758 by a Mr. Wilcke, that a thunder cloud, in sweeping at low elevation over a forest, not unfrequently appears to lose charge without the occurrence of lightning. The under surfaces of such clouds at first present a serrated or tooth-like appearance, which gradually disappears, the teeth retreating into the cloud, and finally the cloud itself rising away from the forest.

In such cases the numerous points on the branches of the trees present facilities for the brush discharge on an extended scale.

To illustrate this action, an experiment was made by Franklin, as follows: A very fine lock of cotton was suspended from the conductor of an electric machine by a thread, and other locks were hung below it; on turning the machine the locks of cotton spread forth their fine filaments like the lower surface of the before mentioned thunder cloud; on presenting a point which was connected to earth below them, they shrank back upon each other, and finally upon the conductor.

But to return to the lightning. Just as a certain amount of water falling through a difference of level produces a definite amount of energy, so a certain amount of electricity falling through a difference of electrical potential produces a definite amount of energy. It is known that if p be the potential and q the quantity of electricity in a flash, the work done during the stoke is $\frac{1}{2}qp$. Now the duration of the illumination of a stroke is rather less than the 10,000th part of a second, and although q is small (Faraday said not more than would decompose a single drop of water), p is so enormous that the flash is often capable of decomposing a million drops of water in series. The potential can be calculated approximately, because it is known that 10,000 volts will spark across a little more than half an inch at ordinary atmospheric pressure; and, as the sparking distance varies as the square of the potential, a flash of lightning 1,000 feet long must be impelled by an electrical potential of $1\frac{1}{2}$ millions of volts or thereabouts. This is only approximately accurate, because the mean atmospheric pressure would be less than that at the earth's surface, and therefore a correction should be made, as the pressure of the atmosphere decreases very rapidly with altitude, and the sparking distance increases very rapidly with decrease of atmospheric pressure. The work $\frac{1}{2}qp$ done by a flash of lightning is used in the disruption of the air, in the destruction of non-conducting solids that obstruct its path, in heat, in light, and in chemical decomposition. Ozone is always produced during thunderstorms.

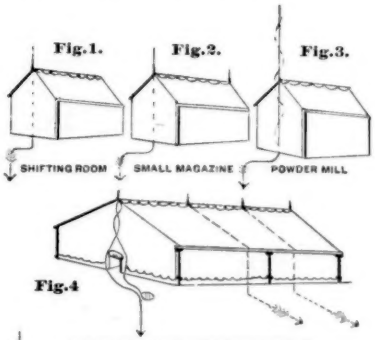
All that can be done to protect buildings from its destructive action is (first) to attract the lightning to another spot if possible, and (second) to arrange that even if the building be struck, the work shall be given out at other portions of the path of the stroke. To do this it is necessary to provide a sufficient conducting channel or channels to convey the electricity past the buildings from the air to the ground.

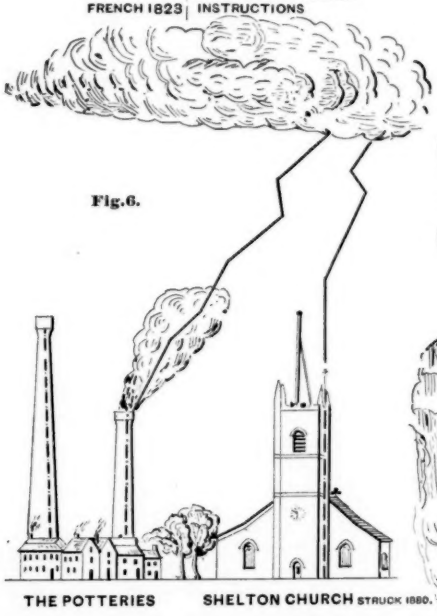
Firstly, let us examine the methods which have been pursued for attracting lightning away from the building which it may be desired to protect. The French Académie des Sciences has issued information concerning lightning conductors on different occasions, the several instructions having been the results of the labors of various Commissions of celebrated physicists.

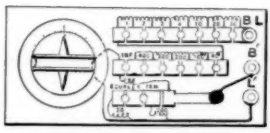
The first instruction, 1823, with Gay-Lussac as reporter, the rule is laid down that *a conductor will effectually protect a circular space whose radius is twice the height of the rod*, and it is stated to be in accordance with calculations made by M. Charles.

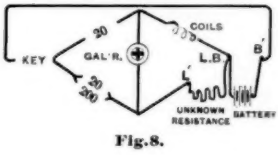
Accordingly we afterwards find in the same instructions that magazines should be protected in the manner shown on Fig. 5, the wording being: "The conductors should not be placed on the magazines, but on poles at from 6 to 8

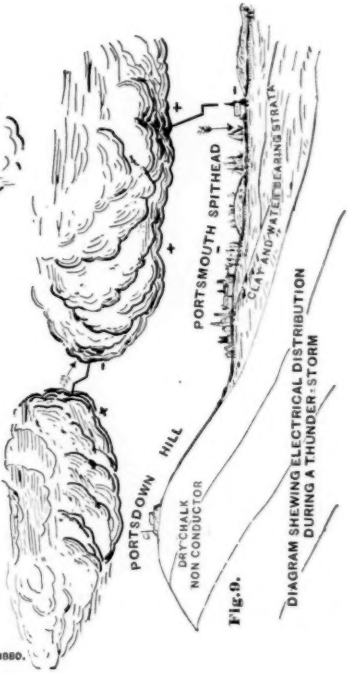
eet distance. The terminal rods should be about 7 feet long, and the poles be of such a height that the rod may project from 15 to 20 feet above the top of the building. It is also advisable to have several conductors round each magazine."











In 1854, however, the next Commission, with M. Pouillet as reporter, no longer supported this rule. The report says:

"At the end of the last century it was a generally accepted opinion that the circle protected by a conductor possessed a radius equal to twice the height of the point. The Instruction of 1829 (Gay-Lussac, *rapporteur*) having found that practice established, adopted it with certain reservations. . . . These rules . . . rest on much that is arbitrary." . . . "and they cannot be laid down with any pretense to accuracy, since the extent of the area of protection in each case is dependent on a multitude of circumstances."

It is the more necessary to make this quotation, because an attempt has recently been made by Mr. Preece to revive the theory in a modified form. In a paper which he read before the British Association last year he attempted to prove that—

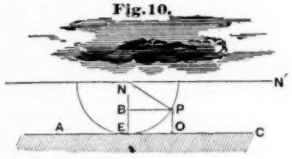
"A lightning rod protects a conic space whose height is the length of the rod, whose base is a circle having its radius equal to the height of the rod, and whose side is the quadrant of a circle whose radius is equal to the height of the rod."

His argument was similar to, but not of such general application, as that used by M. Lacoine in a somewhat remarkable paper read 20th June, 1879, before the French Société de Physique, from which the following is extracted:

"Experience shows that a thunder bolt has a tendency to fall on the metallic portions of a building. If then, by the assistance of a lightning conductor we are enabled to protect a certain metallic surface, much more therefore will the same conductor protect the same surface if non-metallic.

"Let N, Fig. 10, represent a thunder cloud situated over the surface AC to be protected. Assume that the cloud is at such a distance from the point P of the lightning conductor PO, that the circle described from N as center with NP as radius will be tangential to the surface AC. Then the cloud will be equally attracted by the points P and E,* because these points are at the same potential, this rule having always been admitted in all the instructions of the Académie Française. Consequently every point on the surface AC within the circle with radius OE will be protected, but every point outside E towards A would be unprotected.

Fig. 10.



"Hence the radius of protection $r = \sqrt{NE^2 - NB^2}$, NE being the height of cloud above the ground, NB being the height of cloud above the conductor.

"It is enough, then, to know the height of the thunder cloud, to know the radius of action of a certain conductor.

"By several years' observation, and by direct measurement, the average height of thunder clouds could be obtained, and the mean value of r for any given conductor deduced therefrom."*

Mr. Preece does not work out any such formula, but bases his rule on an assumption that a thunder cloud would never be nearer to the earth than the height of the lightning rod. This is open to question, as very low-lying thunder clouds may be driven by the wind into the neighborhood of lofty conductors that command the clouds, and this is corroborated by a case recorded in Mr. Anderson's excellent book on lightning conductors, page 67, where the belfry of an edifice, 115 feet high, "remained standing out clear above the electric cloud" whence issued lightning that killed two priests near the altar of the church. As a single application Mr. Preece's rule comes at once from M. Lacoine's formula.

It is perhaps important to bear in mind these theories concerning the area of protection given by conductors, when it is necessary to fix a few conductors on buildings of considerable extent, such as barracks, hospitals, &c., but sufficient reliance cannot be placed upon the rule to enable us to consider the protection to

* This is open to doubt; the electrical charge on the cloud is attracted by the induction of an opposing *surface*, the total attraction being proportional to the sum of the tubes of force existing between the two opposing surfaces, charged by inductive action. To assume that the charge on a thunder cloud is concentrated at a single point is not in accordance with the circumstances of the case in nature.

Faraday's experiments have conclusively proved that static induction polarizes the particles or molecules of the interposing di-electric, and that dynamic currents tend to traverse the same by disruptive discharge in the direction of the said polarity.

Assuming therefore that a lightning flash from the charged surface NN′ occur at N, it will have a tendency to follow the direction NE rather than the alternative route NP, because polarity exists between NE to a greater extent than between NP.

This consideration will cause the theoretical circle of protection advocated by M. Lacoine to be considerably diminished when the charged cloud lies low, but when the cloud is at a considerable altitude NP becomes more nearly normal to the surface AC, and more nearly parallel to the direction of polarity of the atmospheric particles.

* As the height of thunder clouds varies enormously, the values for r would range between proportionately wide limits, and the *mean* value of r obtained by M. Lacoine would seem to possess no definite or practical utility. If, however, the observations were directed to observing the minimum altitudes of thunder clouds in each locality (the altitudes will be found to vary with the locality), the smallest areas of protection given to conductors there situated could be approximately established.

magazines, as shown on Fig. 1, and already alluded to, as efficient.

The area of protection afforded by a conductor depends much more upon the efficiency of the earth connections than upon the height of the terminal point, and in proof thereof many instances might be cited. For example, in the case of Shelton Church, in the Potteries, which was struck on the 10th June, 1880, the tower, about 16 feet square, is surrounded by four pinnacles 16 feet above the roof, which is nearly flat and covered with slates, with lead guttering and ridges. From the center of the roof springs a large flagstaff, about 40 feet high (see Fig. 6), secured to the tower in the upper chamber 20 feet below the roof by large cross beams unconnected, except by stone work, with the clock-works, bells, and gas pipes in the chambers of the tower. A copper wire rope $\frac{3}{8}$ inch diameter is fitted to one pinnacle and taken direct to earth. Although the flagstaff projects some 20 feet above the conductor, and is distant only 10 feet, a very heavy stroke of lightning, which caused much alarm, and which was seen to fall upon the tower, struck the conductor, knocked the point slightly out of the perpendicular, and passed off by it innocuously. In this case a good conductor, well connected to earth, protected something higher than itself, but not well connected to earth.

Again, Sir William Snow Harris mentions a chimney at Devonport which, although provided with a conductor, was struck on the other side, and shattered down to the level of a metal roof below. Here the conductor must have been badly connected to earth, and was useless.

Moreover, the safe area rule may be upset in practice by all sorts of accidental circumstances. Thus, a house within the theoretical circle of protection given by a church spire close at hand might be struck if the line of least resistance from cloud to earth were afforded by a column of rising smoke from the kitchen fire, and the shorter of the two chimneys in Fig. 6 would most assuredly be struck, for a similar reason, although it is within the theoretical cone of safety of the taller chimney as fixed by Mr. Preece.

In short, if thorough protection be desired for any building it is necessary to put a conductor or conductors upon it.*

Let us now examine the manner in which conductors should be applied.

Churches and dwelling-houses of ordinary dimensions, factory chimneys, monumental columns, &c., need but one conductor led from the most lofty point to the ground, to which a thorough efficient earth connection (to be described presently) must be given. As a rule it is the best plan to fix the conductor externally, in which case it should be connected to all *external* metal surfaces, but *not* to any masses of metal wholly within the building. It should be fixed to the exterior by strong clamps of iron or other metal, and provision should be made for its expansion and contraction due to differences in temperature. It should be continuous from top to toe. It should possess a proper amount of conducting power per unit of length.

As regards the last mentioned and most important matter of conductivity, the last French instructions, dated 14th February, 1867, state that there is no case on record where lightning has fused a square bar of iron having a side of 0.6 inch, or a section of 0.36 □″—and square iron conductors 0.8-inch side are recommended, which gives a section of 0.64 □″. Also Sir William Thomson considers that a round iron bar 1″ diameter would form a very safe protection for magazines; this would be about 0.77 □″ sectional area. It would appear that continuous iron conductors weighing 6 lbs.

* A lamentable result of the practice of placing lightning conductors distant from a building occurred at Compton Lodge, in Jamaica, the residence of J. Senior, Esq. A lightning rod, of small dimensions, of iron, had been set up within 10 feet of the south-east angle of the building, as used to be the practice with gunpowder magazines, on the assumption that the rod would attract the lightning and secure the building. So far from this, the building itself was struck in a heavy thunder storm, 28th July, 1857. The southeast angle was shattered in pieces; the escape of the family appears to have been miraculous; whilst the lightning rod, 10 feet distant, remained untouched. If this building had been a deposit of gunpowder, it would certainly have blown up.

Sir Wm. Snow Harris said: "To detach or insulate the conductors is to run away from our one principle, which is, that the conductor is the channel of communication with the ground, in which the electrical discharge will move in *preference to any other course*. To detach or insulate the conductor is to provide for a contigency at once subversive of our principle. Is it possible to conceive that an agency which can rend rocks and trees, break down perhaps a mile of dense air, and lay the mast of a ship weighing 18 tons in ruins, is to be arrested in its course by a ring of glass or pitch, an inch thick or less, supposing its course were from any cause determined in that direction?"

per yard would be quite safe, as shown in the following table:

TABLE A.

	Iron conductors.		
	Side.	□″	lbs. per yd.
Limits of safety—French instruction.....	□ 0.6″	0.36	3.6
Conductors recommended by ditto—			
from	□ 0.75″	0.56	5.6
to	□ 0.8″	0.64	6.4
Sir William Thomson recommended.....	○ 1.0″	0.77	7.7
New W.O. instructions..	..	0.8	8.0
Now proposed for general purposes.....	..	0.6	6.0

Now iron has about one-seventh, and good commercial copper about four-fifths of the conductivity of *pure* copper. Hence iron has about one-sixth conductivity of good commercial copper. A safe conductor in good copper must therefore weigh 1 lb. per yard.

It is, however, inconvenient to specify for a conductor either by sectional area or by weight per yard, because different samples of metal, and especially of copper, vary considerably in their conducting power. See Table.

Table of conducting power of different descriptions of copper:

TABLE B.

Pure copper.....	...100
Lake Superior.....	98.8
Commercial.....	92.6
Burra Burra.....	88.7
Best selected.....	81.3
Bright wire.....	72.2
Tough.....	71.0
Demidoff.....	59.3
Rio Tinto.....	14.2

Temp. about 15° C. or 60° F.

Imagine a conductor made of Rio Tinto copper(!) No doubt many exist.

A limit of electrical resistance per unit of length should therefore figure in any contract for a lightning conductor, and for the conductors already recommended this limit would be 0.3 ohm per 1,000 yards, or 0.03 ohms per 100 yards, at 60° Fahrenheit or 15° C.

This would be obtained from iron wire rigging ropes weighing 6 lbs. per yard, or from copper (equal to 80 per cent. pure in conductivity) ropes weighing 1 lb. per yard.

When two "earths" are used, and the conductor is carried up one side and along the ridge and down the other side of the building to be protected, it is evident that the conductor may be reduced in power by one-half, but no further reduction can be made when a still greater number of "earths" are used, because the lightning may strike the system of conductors at any point. A 3-lb. iron (or a half-pound copper) rope is therefore the smallest that should ever be used in any situation.

There is much difference of opinion as to whether iron or copper is the better material for lightning conductors.

The French use iron almost exclusively, and Sir W. Thomson prefers it to copper.

For the same money the same conductivity can be purchased in either metal (iron being one-sixth of the price and one-sixth of the conductivity of copper), and iron has the following advantages:

(*a*) The mass of an iron conductor being greater than that of a copper conductor of equal conductivity, it is heated less by a given current of electricity.

(*b*) The fusing point of iron (2,786° F.) is much higher than that of copper (1,994° F.).

(*c*) Iron is more constant in its conductory power than copper of different samples.

(*d*) A conductor made of iron is not so liable to be stolen as copper, and being so much the stronger is therefore less liable to be broken, accidentally or otherwise.

(*e*) A copper conductor if connected to a cast iron water supply pipe (to form an "earth") produces galvanic action, to the damage of the pipe.

On the other hand, a copper conductor lasts longer in smoky towns or near the sea shore, where the air rusts iron quickly, and being of much smaller size it does not interfere so much with architectural effects. But Sir W. Thomson has suggested that iron conductors should be treated boldly by architects, and brought into prominence purposely and artistically, and the late Professor Clerk Maxwell recommended that in the case

of new buildings the conductors should be built into the walls. They would then not only be hidden but protected from the weather, from the British workman carrying out repairs, and from the thief.

As regards the liability of iron to rust, galvanizing is in most situations a sufficient protection, and in smoky towns an iron conductor should be painted periodically.

On the whole, therefore, the advantages of iron outweigh those of copper so considerably, that the employment of copper in lightning conductors should be the exception instead of the rule.

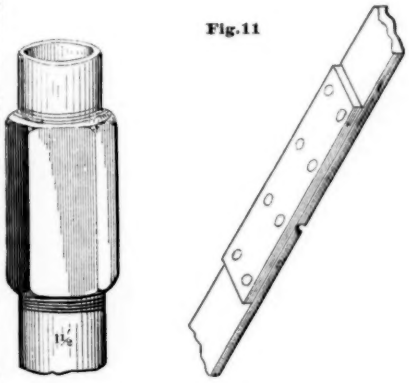
Those who make, supply, and apply lightning conductors in this country, nevertheless, invariably recommend copper; and it is quite difficult to convince them to the contrary.

Another point I notice is that large conductors are always recommended for lofty buildings, and smaller conductors for smaller buildings, and the same for masts of ships. This is unscientific and wrong. The stroke of lightning falling on a short conductor is no less powerful than the stroke that falls on a lofty conductor; indeed the chances are in favor of the shortest conductors receiving the heaviest strokes, if they are struck at all. On costly and important buildings, the proper course to pursue is to increase the number of conductors, and of the earth connections, the limit of electrical resistance between any possible striking point and earth being kept below what is fixed upon as the point of safety, viz., 0.3 ohm per 1,000 yards.

We will now examine the question as to the best *form* of conductor. Mr. Preece has investigated this subject, and by permission of Dr. Warren de la Rue carried out in that gentleman's splendid laboratory a series of experiments on the best sectional form for lightning conductors. The results were communicated to the British Association at Swansea last year. He found that ribbons, rods and tubes, of the same weight per foot, were equally efficient.

The application of rods and tubes necessitate frequent joints, generally made by means of screw collars. I have found by electrical tests that these joints after long exposure to weather offer very high resistances; especially so in copper conductors. For instance, at Tipner magazine a screwed joint in a large tubular copper conductor tested 10,000 ohms, and a riveted joint in a ribbon conductor on a battery in the Isle of Wight 700 ohms. These joints could not be moved by hand, and were apparently quite tight.

Fig. 11



Ribbons of *copper* are now made in long continuous pieces (as much as 70 or 80 feet in one length), and can be applied to irregular architectural outlines, but the joints, although less frequent than with rods and tubes, are open to the same objections. The copper ribbon, however, possesses one decided advantage, viz., that by the introduction of suitable bends, the expansion and contraction from heat and cold can be allowed for. Iron conductors, when in the form of tubes, rods, or ribbons, are difficult to apply, and must possess a number of joints. Moreover, in long conductors, compensators to allow for expansion and contraction by heat and cold have to be introduced. In order, therefore, to obtain with iron the necessary continuity and pliability, it is best to resort to the wire rope, which form is already very generally employed for copper conductors. Pliability can be obtained in several ways:

1. By using small wires.
2. By making the rope *flat*.
3. By using a hemp core with the round rope.

It is not advisable to make the iron wire ropes with very small wires, because oxidation destroys such a rope rapidly if

through carelessness the conductor be left unpainted. A fair amount of pliability can be obtained with a round iron rope 6 lbs. per yard if the wires are about No. 11 B.W. gauge, and arranged in six strands of seven wires each round a hemp core, thus producing a rope about $3\frac{3}{4}$ inches in circumference.

But there are few situations in which two ropes of half the size could not be more readily applied; and I think the double rope, if taken up on one side of a tower and down on the other, in one continuous length, has many advantages.

When a single conductor is desired, the best for general purposes is probably a flat iron wire rope about $2\frac{1}{4}'' \times \frac{1}{2}''$ (11 lbs. per fathom), or $2\frac{1}{2}'' \times \frac{1}{2}''$ (13 lbs. per fathom). The round ropes cost from 21s. to 24s. a cwt., or about 2s. 6d. per fathom for a 12-lb. rope; and the flat ropes 33 per cent. more, or add one-third.

The next question that presents itself is concerning the terminal point, and a good deal of nonsense has been written about it. Points made of silver or of copper, points covered with platinum or with gold, points of so many millimeters in height and diameter, and possessing certain exact forms, have been proposed, and rejected or adopted as the case may be.

The height of the points above the surrounding roof or tower to be protected has also been much debated with very little profit, for to this day many of the rods erected on the continent are made much longer than is necessary.

It is a good plan to carry conductors on lofty rods high above powder mills, flour mills, and petroleum oil wells; but these are exceptional cases, the air close to the buildings being frequently charged so as to be dangerously explosive.

The English practice of using a short rod in most situations is a reasonable plan, the rod being placed on the highest part of the building. The rod should be made of the same metal as the conductor, and the connection formed with bolts and afterwards run in with molten zinc or solder. The weight of the rod per foot should be the same as the conductor. The top of each rod should be provided with several points, (*a*) because the gathering power is increased thereby, and the chance of lightning striking other things in the immediate vicinity of the conductor is proportionately diminished; (*b*) because the top of the rod is less likely to be fuzed when struck, the stroke being divided between the various points; and finally (*c*) because the brush discharge is facilitated.*

Another plan is to carry the wire rope up the side of the rod, which in this case might have one point, the wires being opened out to form a brush-like arrangement just under the point. The wire rope and the rod should be bound together with wire and connected with molten zinc.

We must now pass to the foot of the conductor, and here we enter upon the most difficult part of our subject. The earth connections of a lightning conductor constitute the most important portion of the whole arrangement. If the electrical resistance of the earth connections be high, a conductor, perfect in all other respects, may fail, some alternative and perhaps dangerous route being taken by the lightning discharge. It is difficult to fix the limit of maximum resistance of the earth connections.

The Académie des Sciences recommends an iron earth plate, consisting of four arms on a central bar, or five arms in all, each 2 feet long and of square section 0.8 inch side, thus presenting a combined surface of 2.6 square feet, to be immersed in water in a well that never dries.

Again, Mr. Anderson, in his book before referred to, says that—

"When a conductor is taken deep enough into the ground to reach permanent moisture, the single rope touching it will be quite sufficient. But when the permanency of the moisture is doubtful, it will certainly be advisable to spread out the rope like the fibers in the root of a tree."

Here a few square inches touching permanent moisture is considered sufficient.

Again, Professsor Melseus used three earths for the Hôtel de Ville at Brussels —one the gas main, another the water main, and the third a cast-iron pipe, nearly 2 feet diameter, sunk in a well and giving 100 square feet of surface to the water, which was rendered alkaline

* Sir William Thomson's opinion: "A fork or brush of three or four points at the top of a lightning rod is probably in general preferable to a single point; but of what practical value this preference may be I cannot tell for certain, although I think it may be considerable."

with lime to prevent oxidation. The total surface of these three earth connections amounts to more than $2\frac{1}{2}$ millions of square feet!

As opinions differ so greatly concerning the surface required for the earth connections, it will be necessary before laying down any rule, to give some of the reasons upon which it is based.

I must ask you to examine Table (C) of Resistances, which has been compiled from various authorities, and which deals with such enormous differences that it can only be regarded as approximately accurate.

TABLE C.—*Of Resistances.*

Substance.	Comparative Resistances in Ohms.		Effective Section.
	Copper unity.	Iron unity.	
			Sq. in.
Pure copper....	1.0	..	..
Commercial copper.....	1.17	0.2	0.2
Iron wire.....	7.0	1.0	1
			Sq. ft.
Carbon.....	2,500	360	$2\frac{1}{2}$
Coke, variable with the sample, about...	3,000 4,000	400 600	3 4
Sat. sol. sulph. zinc.....	6,000,000	..	..
Salt (sea) water.	10,000,000	..	10,000
Approximat'y only.....	15,000,000	..	15,000
Water (spring).	2,800,000,000	..	2,800,000
" distilled.	6,754,000,000	..	..
Dry earth (practically).	Infinity.	..	..

We might state the figures against water in this table thus:

The electrical resistance offered by a cylinder of spring water one yard long is as great as the resistance offered by a cylinder of copper of equal diameter, but seven times longer than the distance of the moon.

The study of this table involves some rather curious considerations. Let us call 1 square inch of iron its efficient section* or conductive capability for carrying off a stroke of lightning. Then the efficient actions of carbon, of water, &c., are as shown in col. 4 of table.

Now the practice in the War Department has always been to give joints in conductors a surface of about six times the sectional area of the conductor. This is a very good rule, and is borne out by the French practice, where even with soldered joints, 6 square inches of surface is laid down as necessary at each joint in an iron conductor. An obvious corollary to this rule is that when a conductor is made of two metals (end to end) the joint must have a surface equal to six times the efficient section of that conductor of the two joined which possesses the lowest conductivity. The efficient section of the better conductor ought not in any way to govern the amount of surface of the joint. Thus copper to iron requires a joint of six square inches, the same as would be required by iron to iron. In short, the joints should be made of such a size as to prevent the conductors of lower conductivity being damaged by the lightning.

A copper to copper joint only requires 1 square inch of surface, but it is generally convenient to give more.

Now, the earth connection is really a joint—a very difficult joint to make well, and one that should follow the rules of other joints, *unless we can show good reason to the contrary.*

It is found that increasing the size of an earth plate does not proportionately decrease the electrical resistance. A limit of size is soon arrived at, beyond which it is useless to go. "In the sea this limit is quickly reached."—(Culley.)

Culley states that if a plate containing 1 square foot of surface give a resistance of 174 ohms, a plate of 4 square feet will give 140 ohms, and so on, a reduction of only 20 per cent. in resistance being obtained by quadrupling the earth-plate surface.

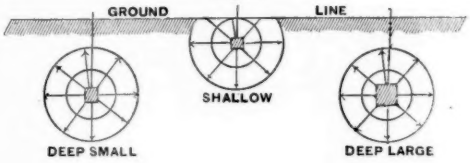
The explanation that suggests itself as probable is that the electric current is distributed through the humid ground by an ever-increasing sectional area (often by an hemispherical surface), thus arriving at the efficient section for a water conductor of two millions of square feet (*see* Table C), at the small distance of 200 yards, or thereabouts,* from the earth plate; and this is borne out by the fact, noted by Culley, that the resistance

* This has already been shown to be rather less than a square inch of solid iron.

* In an arid plain with a dry subsoil, the surface of which was wet by rain only to the depth of one inch, the efficient section of water conductor would not be reached at a less distance than fifty miles.

depends to a certain extent upon the depth at which the plate is buried. Thus, a deep plate would disperse its charge in all directions by an ever-increasing spherical surface up to the limit of a sphere whose radius is equal to the depth of the plate underground, and afterwards by a segment of an ever-increasing sphere, which segment would always in this case be larger than, but would gradually approximate, the atmosphere. These actions are roughly shown on Fig. 12:

Fig. 12



Culley states that the resistance alters with the depth at which the earth plate is buried as follows:—

4	inches	100	ohms.
10	"	90	"
40	"	80	"
80	"	77	"

It would appear, therefore, that little is to be gained by increasing the surface of junction between the earth plate and the earth (1) beyond the amount required to insure that the resistance to earth at foot of conductor is less than the resistance to earth through possible alternative routes in the vicinity of the conductor, and (2) beyond the amount required to prevent damage to the conductor by the flash of lightning when it leaves for earth. It is evidently impracticable to give a surface of some millions of square feet to the earth connections, and if it were practicable, the foregoing considerations prove, I think, that it is not necessary to do so.

The difference in the conductivity of iron and water is so enormous that an intermediary appears to be very desirable. carbon is eminently suited to act in this manner, especially if used in the cheap form of coak and ashes. The minimum effective section for coke is about 4 square feet, the iron which is surrounded by coak should, therefore, have a surface of 24 square feet. Moreover, inasmuch as the contact between an iron plate, of whatever form, and coke loosely surrounding it must frequently be discontinuous, and as the conductivity of coke in a mass composed of loose particles must be very much lower than that of a solid piece, the above surface should in practice be a minimum.

The total surface may, however, be divided if a number of earths be used.

The outer surface which should be given to the coke must depend very much upon the nature of the ground; when the conductor is led into soil which cannot be regarded as permanently damp, the surface of the carbon "earths" must be increased.

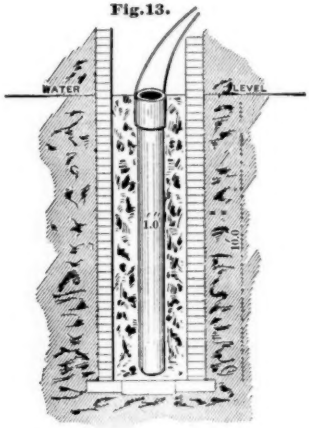
As the surface of the earth connection should vary directly as the resistance per unit of area, an intermediary of coke becomes unnecessary where a conductor is led into salt water; but the conductor should still present a total surface to earth of from 20 to 30 square feet, the amount being divided between the "earths" if several conductors be connected.

Professor Pouillet's Committee, which reported upon the application of conductors to the Louvre in 1854–55 (the said report being adopted by the *Académie des Sciences*), recommended that when permanent water is not found near the surface, two descriptions of "earth" are necessary; firstly, the deep earth connections to permanent water, and secondly, the shallow earth connection to the surface water. This for the following reasons: After a long drought, the "terminating plane of action" (to use Sir William Snow Harris's term) is situated on the upper surface of the deep water bearing strata, the induced charge being consequently collected there. After a heavy rain, however, which thoroughly impregnates the upper strata with water, the "terminating plane of action" is

raised to the surface of the ground, and the induced charge is accordingly collected there. It is evident, therefore, that a perfect arrangement should in many situations provide both for *surface earths* and for *deep earths*. In some situations, however, such as the top of a chalk hill, deep earths would be of little value; whereas in other situations surface earths would be inefficient—in a well-paved town for instance, where the surface water is at once carried off by gutters and drains.

A deep earth connection can be effected in the manner shown in Fig. 13, the well

Fig.13.



being carried down 10 feet below water level in the driest seasons. The diameter of the well may be fixed at 3 feet. It should be rendered alkaline with lime, so as to protect the iron from rust.

The bottom 10 feet should have no mortar or cement in the walls, and should be filled in with blocks of coke. The iron conductors should terminate in cast-iron pipes, offering together 24 square feet of outside surface. The pipe should be galvanized to preserve it from oxidation. The dimensions of the pipe may be, length 10 feet, diameter 1 foot. The pipe may rest on the bottom of the well, in a vertical position. The best way to connect the pipe with the conductor is to have a flange at the top (all ordinary gas or water pipes have such flanges), and to rivet a small cylinder to the inside of the pipe at the upper end, thus forming a ring or annulus, into which the end of the conductor can be introduced, and the space filled in with molten zinc, the surfaces of the conductor and of the pipe having first been cleaned and painted with hydrochloric acid.

In situations where iron water supply pipes are at hand, they can be employed in place of the deep earth connections already described, but great care must be devoted to the connections. The conductor must be laid along the iron pipe for a distance of 4 feet (if an iron wire rope it should be unlaid for this distance), it must then be bound to the pipe with wire, and a metallic connection formed by means of lead, zinc, or solder. The connection should then be tarred and covered with tarred tape to prevent galvanic action.

Surface "earths" should consist of a trench filled with coke and ashes, and carried away from the walls. Clay and other soils which keep the rain-water near to the surface require shallow trerches about 1 foot deep; whereas gravel, sand, or shingle, through which the water penetrates easily, require deeper trenches, say 2 feet deep.

In each case, however, the top surface should be kept on the ground level.

The end of the metal conductor should be carried along the bottom and through the whole length of each trench. This length may in ordinary soils be fixed at 25 feet, and in very porous soils at 50 feet.

The water pipes from the roof of the magazine or building may with advantage be caused to deliver into gutters which lead to the surface "earth" trenches.

The shallow trenches, 1 foot deep, recommended for stiff soils, may conveniently be split into a V shape on plan (the conductor being split also), so that the total side surface may be equal to that given by the same length of deeper trench used with porous soils.

Important buildings and magazines provided with several conductors, may have a few deep "earths," and several shallow "earths," an "earth" of one or the other description being provided at the foot of each vertical conductor, and in order to connect the whole it is advisable to employ a horizontal conductor near the foot of the wall, but above

ground in order that it may be open to inspection, such conductor being carefully connected to all the vertical conductors, and to all the metal water pipes. By this means not only is the cage principle advocated by the late Professor Clerk Maxwell and other physicists embodied, but the earth connections are connected in an efficient and reliable manner.

Sir W. Thomson considers that conductors on magazines should be spaced at intervals of about 50 feet, by which plan no portion of the building would be more than 25 feet from a conductor. This rule has been adopted by the War Department for all large magazines, and a conductor of power equal to an iron rod weighing 8 lbs. per yard has been adopted for single conductors, and of half that weight for all others. A wire rope of 4 lbs. per yard applied *as shown in diagram*, is now considered the best arrangement.

It will be seen that wherever the lightning falls a conductivity equal to, or more than, that of a single large conductor will carry the stroke off to earth.

Small magazines can be protected by one rope led to a deep "earth" at one end and to a shallow "earth" at the other, as shown on diagram.

Powder mills must be provided with lofty conductors, to guard as much as possible against powder dust in the air being ignited by the stroke.

As regards the inspection of lightning conductors, opinions vary greatly, and it was mainly in order to obtain a report on this matter that I was ordered last summer to inspect a number of conductors on magazines in the Portsmouth district. I will read a few extracts from my report. (See Appendix I.)

Before concluding this paper, I may observe that the principal object has been to prove the following points:

1. That iron is the best metal to use in conductors.
2. That wire ropes are more easily applied than rods, ribbons, tubes, &c.
3. That conductors should be continuous, and that all unavoidable joints should be soldered.
4. That conductors should be specified in terms of electrical units.
5. That lofty conductors require no additional conductivity per unit of length.
6. That high lightning rods are only required in exceptional situations.
7. That several points are preferable to a single point.
8. That greater surface than is usual with present practice should be given to earth connections.
9. That both deep and shallow earths are required.
10. That periodical inspection is most important.
11. That the history of conductors and of former tests should be carefully recorded.
12. That electrical tests may then be of value.

APPENDIX I.

I have to report that, in accordance with instructions, I have made nearly 500 tests, and have inspected the whole of the lightning-conductors on fortifications in the Portsmouth and Gosport Divisions of the southern district, and have come to the deliberate conclusion, after a careful study of the subject, that *with the lightning conductors erected as they are at present by W.D.*, electric testing is of small value.

The fact that the conductors on one building test lower than the conductors on another building certainly points to the inference that the earth connections in the former case are of superior efficiency; but it does not prove it. Moreover, although the tests are sometimes of value to the inspector *when he knows the details of the earth connections from the office records*, the tests taken by themselves are frequently positively misleading, so far as the earth connections are concerned. As regards the conductors themselves, above ground, high resistance tests do not prove inefficiency when the W.O. rule that the surface of the joint shall be at least six times the sectional area of the conductor is strictly adhered to; and in this view I am borne out by Sir William Thomson's opinion, which now lies before me, viz., "that although it would be desirable that the joints should be considered and run in with lead, so as to make sure of absolute contact, at the same time it is to be remarked that the great resistance at imperfect joints is not detrimental to the lightning conductor, because, when a discharge takes place, the imperfect joint is bridged across, and the resistance, which is very great when tested by a feeble current, becomes practically annulled in the electric arc during discharge."

Dr. De la Rue also writes to me and says:—"The resistance of many megohms would offer an insignificant obstacle to a lightning discharge, on account of the extremely high potential of a thunder cloud. Consequently, a conductor would be quite efficient, although offering a megohm resistance."

The opinion that lightning conductors with large surface joints rre efficient, although offering high resistance at the joints, is also substantiated by the well-known action of plate paratonnères, as applied on the flanks of electric telegraph stations, to protect the instruments therein from the effects of strokes of lightning upon any portion of the line. These paratonnères consist of plates, in most patterns smaller than the flat joints of lightning conductors, and paraffined paper is interposed between the plates the more thoroughly to insulate the lower plate from "line." A number of these paratonnères are in store at Woolwich, and they each test from 3 to 40 megohms of resistance; yet in practice a flash of lightning is always found to pass across them to good "earth," in preference to the alternative path offered through the telegraph instrument, usually of less than 2,000 ohms. It is therefore quite erroneous to suppose that lightning always passes to earth by those paths which, *to ordinary voltaic current*, test lowest. It, however, does pass to earth by those paths, which *to a current of its own potential*, would test lowest. . . .

With regard to the conductors now existing on our magazines and fortifications, and which have been erected for the most part on sound principles, and which have never yet failed, it would appear that the periodical inspection should be performed by a thoroughly competent inspector who has studied the subject. He should be provided with drawings and record plans, and every information that can be afforded of each and every conductor in the district to be inspected. The information concerning the earth connections should be most minute and exact. He should also be provided with a light equipment for making such electrical tests as he may find necessary. If this were done, my recent experience would point to the conclusion that the etectrical tests would form the least important portions of his periodical reports. . . .

As far as my own experience has gone, it would seem that our conductors are, with few exceptions, as efficient now as when they were first put up; but the earth connections of most of the conductors are and always were considerably below the standard. . . .

Although the lightning conductors at present on our magazines and forts are no doubt, so far as the conductors themselves are concerned, efficient, their efficiency could nevertheless be guaranteed with greater certainty if more modern practice were followed. . . .

The adoption of modern practice would at once make electrical testing of considerable value, because with *unbroken continuity* and the *best earth connection*, all conductors would test at a very low figure indeed, unless out of order. An economy would also be effected on all new works, because metal pipes and rods, with costly sliding joints to allow for expansion and contraction, would no longer be required.

As regards the testing of conductors: a few tests were taken with the three-coil galvanometer, but with no satisfactory results, as the instrument is not sufficiently accurate when used as a measurer of electrical resistance. An attempt was then made to test by means of the "earth" cells produced by the earth of the lightning conductor, which was always either of copper or iron, and a test earth of iron or copper. This gave promise at first of becoming a good test, the astatic galvanometer being employed, but the method was soon discarded from want of accuracy. It is, however, useful for the tester sometimes to discover the metal of the earth connection of a conductor, and the above method can then be resorted to. . . .

A quarter-mile of the light insulated wire for Engineer mountain equipment (60 lbs. per mile) was cut up into three pieces, each 110 yards long and 4 ohms resistance, and two pieces each 55 yards long and 2 ohms resistance. This wire was found to answer well, and being so light, could be carried over a man's shoulder without any difficulty for considerable distances. . . . .

Two small plates (one copper and one iron) were used, their dimensions being 7 inches wide and 8½ inches long; they were of oval shape, and made of quite thin metal. A lip was formed at the top, and a hole punched in the plate 2 inches below it; a 2-foot piece of Navy demolition cable was then brought through the lip, passed through the hole, the wires cleared of insulation for 1½ inches, and the ends spread out like a fan and soldered to the plate. The lip at the top was then firmly hammered over the covered wire until it held the wire tightly. The other end of the piece of core was then stripped and the wires sweated together ready for insertion into a brass connector when required.

A number of resistance tests having been taken with the P.O. pattern resistance coils, an astatic, and service six-cell test battery, it was found that the tests usually ranged below 200 ohms; and I designed an instrument to test these resistances with approximate accuracy up to 200 ohms, and to measure roughly up to 2,000 ohms, the bottom plug being placed in the "×TEN" hole when measuring the higher resistances. The whole arrangement weighs less than 6 lbs. when the battery is charged; its dimensions, moreover, are only 9″x5½″x6″ over all, and the method of using it can be taught to any intelligent man in a few minutes. The instrument shown on Fig. 7 is the latest and improved pattern, and has a range up to 1,110 ohms, when testing direct by steps of 1 ohm; and to 11,100 ohms by steps of 10 ohms, when using the multiplying hole marked "×TEN." In testing a conductor's "earth" the wire to the conductor would be taken to terminal L′; one pole of the battery and the wire to the test earth plate to terminal BL, and the other pole of the battery to terminal B′; the plugs on the upper row of brasses would then be moved about until no deflection is produced upon the galvanoscope on the battery key being pressed down, the bottom plug being placed in the "EQUAL" hole. If, however, the resistance to be found is more than 1,110 (shown by above trial) the bottom plug is moved to the "×TEN" hole, and a balance obtained and recorded.

The silver chloride battery is used on account of its small weight, and when kept in a dark

box it is fairly permanent. All the connections are permanently made, which simplifies the testing very much indeed. These connections are all shown in the diagram, and will be understood by any electrician. The sketch on Fig. 8 shows the electrical arrangement a little more graphically. Everything is done permanently, except the connection of the unknown resistance x between terminals L′ and BL, the plugging at R, and the insertion of the EQUAL or ×TEN plug. The tests taken in the Isle of Wight were performed with the instrument It saved much time, being very rapid in action and easily set up. It has also been checked for accuracy by a series of tests at Woolwich with satisfactory results.

A special clamp was found to be useful in connecting the test wire to the conductors, a small clean spot being produced by a file for the end of the screw to seat upon. When the leads had to be connected for long stretches the naval pattern brass connectors were used.

APPENDIX II.

Extracts from a Memorandum by Colonel H. Schaw, R.E., 1879, *on Lightning Conductors.*

"The testing of the electrical resistance of a system of lightning conductors will generally present great difficulties, because the ordinary means of allowing for expansion and contraction by slotted joints destroys the metallic continuity of the conductors, and introduces a variable resistance of oxides and foreign substances between the slipping surfaces.

This resistance will generally be very much in excess of that of the whole length of the conductors; it is, however, of little or no consequence when opposed to electromotive force of such high tension as a lightning discharge, which will easily pass the obstruction as exemplified in the form of lightning protector used by Messrs. Siemens for electric telegraph stations, which is formed by two brass plates with roughened surfaces placed face to face, but prevented from coming into contact by a thin strip of mica.

If the line wire is struck by lightning, the discharge takes place to earth through the protector, the two plates becoming oppositely charged by induction, and a spark passing between them. . . .

The ordinary currents have not a sfficient tension to pass the air space in the lightning protector, but go to earth through the more circuitous route of the instrument.

The test by simple inspection would seem to be the best for the conductors above ground. A resistance test could only be applied with advantage where there were no slip joints, and where the conductors were difficult of access.

As regards the earth connection, simple inspection may frequently be the easiest and most satisfactory test also. It is known by experience that 10 superficial feet of metallic conductor in contact with *wet earth or water* is sufficient to carry off safely any discharge of lightning. If then we can by inspection ascertain that in *dry summer weather* we have such a connection we may be satisfied. Should it be difficult to inspect, then the electrical test should be used, and I should prefer the Wheatstone balance test. . . .

It might happen that the connection between the conductor and the plate, or tube, or mass of metal forming the earth was imperfect, owing to oxidation. In such a case the resistance would appear considerable, yet in reality the connections might be practically good as regards lightning, as a spark would pass from the conductor to the plate, &c., and from its large surface of contact with water it would escape freely and harmlessly...

Hence I consider that in all possible cases inspection is the best test, but that electricity carefully used may assist the inspection in cases where the earth connection is difficult to get at.

It is most necessary that tests or inspections of earth connections should be made at the driest time of the year. In wet weather they must always be unreliable.

In rocky or very dry sites good earth connections are most difficult of attainment. . . .

I do not think that tests made by weak currents are of any very great value in deciding on the resistance of earth connections intended to carry off a great charge of electricity at one instant of time, as in the case of a lightning discharge. H. Schaw, Colonel, R. E.

24th January, 1879.

P.S.—Were all systems of lightning conductors arranged so that expansion and contraction might be allowed for by S bands of flat iron instead of by slip joints, and all other joints welded or soldered, electrical resistance tests could be applied without difficulty, and I consider this would be very desirable.

It is a remarkable fact that there was only one instance of accidental failure in the automatic drop of the Greenwich time-ball during the whole of the past year.

On June 15, the *Nature* reports that M. Marcel Deprez delivered, in the large hall of the Conservatoire des Arts et Métiers, Paris, a lecture on the transmission of electricity to great distances. He proved that magneto-electric machines could be moved through four kilometers of German silver wire, the resistance of which was 12 times that of a similar wire of copper. He also declared that he could go almost any length in diminishing indefinitely the diameter of the wire of his dynamo-magnetic machine, and that it is by resorting to large dynamos that he will be able to produce a current sufficiently powerful.

ON THE MAGNETIC "AFTER-EFFECT."

By FELIX AUERBACH.

From "Wiedemann's Annalen," for Abstracts of the Institution of Civil Engineers.

In all the magnetic theories of Poisson and others, the magnetic state of a body at any time is supposed to depend merely on the magnetizing forces at this time.

Under "after-effect" are understood two kinds of phenomena. The one, the changing magnetic state of a body during the action of a constant magnetizing force, or after the force ceases to act; the other being the dependence of magnetic state not merely on the amount of magnetizing force acting at the time, but on the amounts of these forces which acted before this time, and the previous conditions of the body. As to after-effect in elastic phenomena and in magnetism, the author mentions the work done by Kohlrausch, Fromme, Meyer, Warburg, and himself.

The author has already considered, in a previous communication, "after-effect" of the second kind. The general question which remains to be answered is, how does the present magnetization, m, depend on the magnetizing forces $J_1 \ldots J_p \ldots J_q$, respectively, which have acted at a previous time when the magnetic conditions were $M_1 \ldots M_p \ldots M_q$, and this he has tried to answer. In the present paper, as the question is a complicated one, he gives a qualitative answer, reserving for a future communication his numerous tables of experimental results and formulæ. The arrangement of his experiments was the same as in his first researches. The body operated upon was a hollow soft iron cylinder, $5\frac{1}{2}$ inches long, 0.69 inch in diameter, magnetized by means of a coil of wire. The following are some of his results: If the magnetizing force i, following on a condition of no force, would produce the magnetization m_o; then, if besides the force i acting at present, a series of forces, $J_1 \ldots J_p \ldots J_q \ldots$ acted previously, instead of the magnetization m_o, there would now be the very different magnetization m, the difference between them being the "after-effect" of the previous forces. J_1 is of importance in maintaining after-effect, so long as all the succeeding values of J lie belween J_1 and i, but after any subsequent value of J lies outside these limits, it may be considered that no after-effect is due to J_1. Again, of two previous forces which lie upon different sides of i, the second alone is of importance if it lies farther than the first from i; in every other case they are both of importance in determining the value of m; it is never the case that the first alone is useful. Permanent magnetization of steel is a special case of "after-effect," and its laws are merely special cases of general laws. Just as it was found that, when forces followed one another discontinuously, certain intermediate forces are of consequence, so it is found that, if the force alters continuously, the rate of change is without influence on the after-effect—at least, the influence is small in comparison with the after-effect itself. If the magnetic force be increased suddenly, a magnetization results which decreases in time, at first quickly, then slowly, and approaches a constant value, which is, however, greater than the constant value produced after very slow production of the same force. The rate change of force is only of influence on the "after-effect" of the second kind, when it is so great that it causes an after-effect of the first kind. Lastly: The magnetic after-effect is in no case very small in comparison with the magnetic effect itself, although it is always less, but between a value equal to the effect itself and zero it may have all values. It is not easy for it to approach the value zero. The author concludes by saying that no theory of the cause of the second kind of after-effect can be worked out till the phenomena of the first kind of after-effect are thoroughly mastered.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—At a meeting of the Society held on June 21, a paper by O. Chanute, member Am. Soc. C. E., subject, "Uniformity of Railway Rolling Stock," was read and discussed. A meeting of the Society was held July 5, 1882. The succeeding meeting will be September 6, 1882.

ENGINEERS' CLUB OF PHILADELPHIA.—Regular Meeting, June 17th, 1882.

President Rudolph Hering in the chair.

Mr. John T. Boyd described a Shrinking Gauge, which was designed by Mr. Brown, general foreman of the works of the Hartford Engineering Co., and enables the average lathe hand to make the "shrinking fits," instead of placing the latter in the hands of one or two first-class machinists in the establishment, which is probably the practice in the majority of machine shops throughout the country. The gauge resembles, in miniature, an arm swivel for a tension rod, in which one of the bolt-ends contains a fine thread screw. The three screws have each a milled head jamb-nut, to maintain them in position when adjusted.

To use the gauge, the diameter of the hole in the wheel-hub, collar, coupling, or lever boss, as the case may be, is first obtained by bringing the inside ends of the large screws in contact, and locking them securely with their respective jamb-nuts; then running the fine thread screw out until it calipers or gauges the required distance; finally locking the last named screw.

One of the large screws is now unlocked and moved away from the other a distance determined by placing between the inside points of the large screws, and jambing the same, a thin strip of metal, which is in reality the measure of the shrinkage or the difference by which the diameter of the shaft is to be greater than the diameter of the hole. The proportion by which these differences are made is obtained by experiment only and varies with the sizes and materials.

The gauge is well made of steel, hardened where necessary, is light and easy to use, and has a complete set of shrink measures, properly marked, for different diameters of shafts.

Mr. Geo Burnham, Jr., described a wood screw in which the thread, instead of being cut, is formed by passing the blank through a series of rolling, working against stationary, dies. The first set forms a slight ridge only, the second deepens it, and so on until a perfect thread is formed. The thread of the finished screw is slightly larger in its outside diameter than the unthreaded neck of the screw, and the point is turned conical and left unthreaded, thus differing from the ordinary cut screw, in which the thread continues to the extreme point. The object of this construction is to adapt the screw to the present mode of using it in soft woods; that is, driving it part way home before using the screwdriver. Bolts are also made in the same way, the thread appearing to the eye as perfect as a cut thread. It is claimed that a bolt made in this way is ten per cent. stronger in the thread than a cut bolt.

Mr. Wm. A. Ingham made some remarks upon experiments in jigging ore. After premising that there are two classes of jigging machines—one in which the tray with the ore is moved up and down under water, the other in which the tray is fixed and the water is forced up and down through the ore bed—he proceeded to comment upon the difficulty he had experienced in obtaining from the books fixed data for the construction of a jig of the second class. He found that great variations prevailed in the practice at different concentrating works. The speeds of the water piston ranged from 48 to 200 per minute and the stroke from 4 in. to $\frac{1}{2}$ in. There were similar variations in the sizes of the particles operated on, in the length of the screen, in the degree of the inclination of the bed, and in fact the best practice varied at every point.

In the face of such diversities, he was obliged to construct his jig with all its parts adjustable, and determine for himself by a series of trials the conditions best adapted for his work. He soon found that, the other parts remaining fixed, the results could be varied as required by merely varying the piston speed and stroke, and that a high speed was necessarily connected with a short stroke and vice versa. He concluded by promising to prepare a paper on the subject at some future day.

The following Report was presented:

The Committee of Award of "the Prize offered by a Member of the Club, May, 1881," beg leave to report that they have carefully considered the papers submitted for competition, and have awarded the prize of $50.00 for the paper upon a subject strictly in Mechanical Engineering, to Mr. Wilfred Lewis, of Philadelphia, for his paper on the "Application of Logarithms to Problems in Gearing;" and $50.00 for the paper upon a subject of Civil Engineering, to Mr. P. A. Baermann, of West Troy, N. Y., for his paper on "What Thickness of Metal Should be Given to Cast Iron Pipes Under Pressure;" these being the two papers which, in the judgment of your Committee, conformed the most nearly to the requirements indicated by the Rules heretofore published for the guidance of the Committee. All of which is respectfully submitted.

FRED. GRAFF, *Chairman.*
GEO. BURNHAM. JR.
HENRY G. MORRIS.
HOWARD MURPHY,
Secretary and Treasurer.

ENGINEERING NOTES.

THE Select Committee of the House of Commons has passed the Bill authorizing the Solway Junction Railway Company to raise sufficient capital to reconstruct the viaduct across the Solway Firth. The new viaduct will be 1 mile 180 yards in length. The old one was broken down by a mass of iceflows

in January of last year, as described by us at the time. Since then the English and Scotch sections of this company's railways have been altogether disconnected. The new bridge will be constructed under the direction of Mr. Brunlees, C.E., with wrought iron columns instead of cast iron.

CURRENTS IN THE SUEZ CANAL.—By M. de Lesseps.

A series of very careful observations of the tides and currents in the seas near the outlets of the canal, of the tidal waves up the canal, of the prevailing winds, and of the variations in level of the seas and lakes, has been taken from 1872 to the present time. From these observations it appears that the north and northwest winds, which prevail from May till October, raise the mean level of the sea at Port Said and lower it at Suez, producing in September a difference of level of about 1 foot 4 inches, which creates a current, subject, however, to interruption from the tides, in the canal from the Mediterranean to the Red Sea. In the winter the direction of the current is reversed, owing to the prevalence of southerly winds and a consequent raising of the mean level of the Red Sea above that of the Mediterranean, amounting in January to 1 foot. A volume of water is consequently being alternately poured from one sea into the other, amounting in the year to about 14,000,000,000 cubic feet, which, in conjunction with the tides, both annihilate the effects of the evaporation on the surface of the lakes, and help to dissolve the salt deposits in the Bitter Lakes. The rate of flow between Port Said and Timsah Lake varies between 6 inches and 2 feet per second; and between Suez and the Bitter Lakes it varies between 2 feet and 4½ feet per second. These currents do not at all interfere with the navigation. The dissolving of the salt deposits in the Bitter Lakes since they were filled with water in 1869 has produced an increase in the depth of water, and affords a refutation to the notion that if the sea were let into the basins in the African deserts they would soon be converted, by evaporation, into large salt-beds.—*Comptes rendus de l'Académie des Sciences.*

THE WATER SUPPLY OF VENICE.—Venice, a city of 130,000 inhabitants, with factories and a naval station, has been notorious for its defective supply of water of bad quality, even since the construction of artesian wells in the last forty years.

In 1868 two proposals were made to the municipality, one by Engineer Silvestri, to bring a supply from the Sile at Canizzano, the other by a Belgian company to bring water from the Breuta, in both cases through a conduit along the railway. In 1875, five more projects were submitted, one of which, a combined proposal of civil engineers. Ritterbandt, Dalgarius, and Ponti, was accepted. Arrangements for carrying out this work were made in 1879 with a French construction company; the terms of concession being a rate of nearly 1s. 5d. per 100 cubic feet delivered at a height of 164 feet above ground level, a minimum daily supply of 197,180 cubic feet, a storage of 3,530,000 cubic feet, and a duration of concession of sixty years. Finally, some improvements and general modifications suggested by Engineer Fumico were adopted with further alteration, and the works were carried out in general accordance with them by the Societa Veneta.

The supply was taken from the Breuta, above a dam at Stra, and conducted by a channel to the bed of the Seriola, and thence to the filter beds of Morauzani; the supply is 53 cu. ft. per second; but it is proposed to obtain a further quantity from a point higher up the stream. The four filter beds have an aggregate surface of 12,500 sq. ft., the filtering materials being pebbles, gravel, and sand, and the surrounding walls being carefully constructed to prevent saline infiltration from the adjoining salt marshes. The filter beds also act simply as reservoirs when the Seriola water is so pure as not to require filtration. Adjoining the filters is the pumping station, where pumps driven by a turbine raises the filtered water into a collecting reservoir. From this the water is taken in pipes of 2.6 ft. diameter under the lagoons and salt marshes for a distance of about 3½ miles to Venice. The pipes were laid by means of coffer-dams, the beds being pumped dry, and the pipes generally laid in a concrete trench in the bed of the lagoon. At passages under deep channels and canals, that frequently occurred in these lagoons, specially inverted syphons were employed, and a syphon crossing over a bridge in the town was also constructed. The pipes were ordinary cast iron socket pipes, with lead joints, made by the Societa di Marquise and di Terni, weighing in all about 2,550 tons. The reservoir at Venice is built on piles, vaulted and covered with earth; it holds 3,530,000 cub. ft. of water. —*Engineering News.*

ORDNANCE AND NAVAL.

THE 100-TON GUNS.—The four 100-ton guns purchased from Sir W. Armstrong & Co. some time since for £64,000 are still at the Royal Arsenal—the admiration of all the strangers who visit that establishment; but to those initiated in matters of armament, a sad waste of public money. These unwieldy monsters are now relegated to Malta and Gibraltar, and are already obsolete. It is probable that they will never fire a shot beyond those at ordinary practice. Even little of this will take place, owing to the heavy expense of the charge, about £100 per round. Taking all cost into consideration, this sum will barely cover the value of each discharge from these ugly and unprofitable weapons. As showing the way the public money is spent over relatively useless war material, no less than £24,000 have been absorbed in the construction of special shear legs and other appliances for getting these guns into position in our Mediterranean fortresses. In addition to this, the War Department steamers have been specially fitted for carrying the guns out, and will have to undretake two voyages in their conveyance.

Then to the cost of this must be added that of their carriages, which so far has not been made public, probably £10,000 in addition, so that these four guns will cost this country over £100,000. If they were considered by scientific experts to be trustworthy, even this amount would not be grudged by the public. But when it is well known that they were purchased without so much as an adequate trial to test their capabilities, and that one of the same construction, and by the same makers, burst at practice on board the Duillio, the public, we think, cannot be fairly congratulated upon the transaction. Matters with respect to the armament of this country are at present in anything but a satisfactory condition, and the sooner decisions connected with the national armament are delegated to a body of able and scientific gentlemen of known reputation, and who shall be the nation's representatives for this most important matter, the better it will be for the British taxpayer, and the safer will the country be at the time of trial or difficulty. We certainly think that a Royal Commission to investigate into the systems of manufacture, supply, and condition of the national armament should be granted without the slightest hesitation. At the present time the national armament is in the hands of a few, whose only qualifications are that they are military or naval men.—*Engineering.*

THE ARMSTRONG RIBBON GUN.—The firm of Sir W. Armstrong & Co. has recently submitted for trial a breech-loading gun upon a peculiar system of construction. This gun, though differing but slightly in its breech-loading arrangement from those of the Government pattern, is altogether unlike them in general appearance and method of building up. The whole of the piece in rear of the trunnions is built up of steel wire, over which is shrunk ordinary yet thinner coils of great tenacity. The gun's diameter where the charge rests, as compared with that of the War Office construction, is astonishingly small. Its outlines, therefore, form those of a long slim weapon. Yet it is said to be capable of bearing the explosion of 300 lbs. of the slow-burning service powder, with a much heavier weight of shot than that of the 10.4 in. bore Government gun. As a matter of fact, however, the exact weight of shot or shell to be fired with the new gun has yet to be determined upon by experiment. So far, the results have been deemed satisfactory. The weight of the new gun is only 21 tons 4 cwt., yet the diameter of its bore is 10.238 in. Its length is similar to that of the Royal Gun Factories' 10.4-in. gun of 26 tons. Should experiments with this gun prove successful, a new departure in construction will have been taken, and a great step made towards the improvement of our ships and forts. At a future time we will have more to say concerning this gun and its performances. For the present, we are inclined to believe that the construction of the gun does away to a great extent with the present principle of coil shrinking that creates a bursting strain even while the gun is quiescent and free from the effects of the explosive charge. The Royal Gun Factory is devising and constructing various improved systems of breech-loading arrangements. As experience is gained, a fresh departure in the direction of a better apparatus is effected, and it is anticipated that the latest production will altogether throw into the shade its predecessors. A new obdurator is also being experimented with on the principle of M. de Bange, composed of asbestos and mutton fat compressed by hydraulic power into proper dimensions and shape, and then fastened in front of the breech screw. This description of obdurator appears to answer well so far as it has been tried. It seems to hermetically seal up the breech when the explosion of the charge takes place. The life of this form of obdurator is estimated to be that of 200 rounds, at the expiration of which it can be replaced in the front of the breech screw without much trouble. If successful, it will supersede the present form of inverted steel cup loosely fixed to the breech screw head.—*Engineering.*

RAILWAY NOTES.

WE have received a copy of a pamphlet of very considerable dimensions containing "facts from experience" with Cleminson's flexible wheel base-rolling stock. The facts extend over six years of working of the system as applied to carriages and wagons over the greater part of the world, and of gauges ranging from 23½ inches on the North Wales Narrow-gauge Railway, to 6 feet, as on some of the Australian lines. We have already fully described Mr. Cleminson's system as applied to the royal saloon carriage on the South-Western railway and on many other railways, in our impression of the 15th February, 1878, and since referred to its application at home and abroad. The pamphlet shows that the system is working with complete success and economy on 150 railways, consisting of 25 home, 95 foreign, and 30 colonial lines, and on these lines there are running 26 engines fitted on the system and over 4,000 carriages and wagons, while it appears that there are now over 100 engines building on the system and 2,000 carriages and wagons. The advantages of the system are chiefly safety and ease in passing round curves, reduced wear and tear of rails and flanges, and an increased carrying capacity in some cases of 35 per cent., with a reduction in weight of 25 per cent., as compared with rigid axle rolling-stock. By the use of three pairs of wheels on the system, long carriages may be used, as they are completely supported from end to end, and follow curves much more smoothly than the ordinary short wheel base stock. These advantages are, it is plain, being fully appreciated, as besides new stock a good deal of old stock has been altered to the system.

MECHANICAL POWER ON PARIS TRAMWAYS.—Those who have had most experience in the use of steam on the Paris tramways are perhaps least surprised that after about five years' trial the system has been abandoned, and

a return to horse power has been decided upon. It is not too much to say that the design of a tramway locomotive for working in the streets of a city presents more difficult points than the design of any other class of engine, and hence the really satisfactory tramway engine has yet to be made. The objections that are now made to the engines about to be entirely superseded by horses are numerous, and some are equally to be applied to tram-cars hauled in any way; but the real objection to these engines has been the cost of maintenance and working, and the comparative frequency of stoppage by reason of breakdowns, of small or great importance. The Paris company has tried twenty-one different engines, and the results are that horse traction is, on the whole, more satisfactory to the company. This will probably be felt as a blow to mechanical propulsion, and no doubt it will have a retarding effect, but the various causes of failure and the experience gained will form the basis upon which engineers must start anew to make an engine that will stand the abnormal wear due to bad permanent way, dust, mud, frequent stoppages and very short curves, and that can be run without danger by one man. We have several times given some ideas on the construction of tramway locomotives, and, until engines are made with parts and fittings that will be indifferent to dirt and mud, very bad permanent way and short curves, no success will be achieved.

In districts where water is largely impregnated with lime, iron tubes will not answer for locomotives. Lime is quickly deposited on the tubes, and it adheres much more strongly than it would on brass tubes using the same water; in brass tubes a thin scale of $\frac{1}{16}$ to $\frac{1}{4}$ in. thick would be formed, while the incrustation about the iron tubes would, in a few years, completely block up the water space between the tubes; when this takes place, it is impossible to keep the tubes at the fire-box end tight. To prevent the sediment from adhering to the iron, paraffine oil is recommended, even where brass tubes are used; about three pints for every 1,000 miles run, put into the boiler the evening before washing out on the following day, is mentioned as the quantity. Being free from acid, this oil is safe to use.

The prospect of a railway through the heart of Australia, from Fort Darwin to Adelaide, is already stimulating enterprise and speculation. Five hundred miles of this railway, from Adelaide northwards, the *Colonies and India* says, have already been completed; 100 miles from Port Darwin, in a southerly direction, are likely to be soon authorized by the Government; and the construction of the remainder is but a question of time. Another railway, in Queensland, connecting Brisbane with the Gulf of Carpenteria, and possibly ultimately meeting the line from Port Darwin, is also projected, and must have a remarkable effect in developing the resources of the northern half of the Australian continent. With these railways built, those fertile parts of the continent, which have hitherto received but scant notice from the capitalist and the laborer alike, will take rank among the richest portions of the British Empire.

IRON AND STEEL NOTES.

Receipt for Bronzing Iron.—Iron has sometimes to be bronzed for domestic use. The following is a very simple way of obtaining a very good bronze: Mix an equal quantity of butter of antimony and oil of olives; put this mixture on the iron which is required to be bronzed with a brush, the iron having been previously brightened with emery and cloth, and leave it for several hours; then rub with wax and varnish with copal.

Melting Steel by Electricity.—An interesting experiment made by Mr. Siemens a short time ago, in the presence of a large number of practical electricians, is described in a French journal. A number of broken pieces of steel were put in a suitably arranged crucible, with a perforated lid to it, the two currents of the electro-motor terminating in the upper and lower part of the crucible. In fourteen minutes the entire mass of metal was heated, turned red, and liquefied. There was not a single bubble in the mass. The cost of fuel required for this apparatus is very much less than that which would be wanted if the fusion were effected by the direct application of the heat. A considerable saving may consequently be effected in steel works if this process is generally adopted.

The Staffordshire Steel-Making Experiments.—Mr. P. C. Gilchrist, and the Committee of Staffordshire ironmasters with whom he is associated in the conducting of experiments at Wednesbury, which aim at the making of basic Bessemer steel from Staffordshire cinder pigs, have brought their labors to a close. One hundred tons of pigs probably have been blown, and perhaps seventy tons of ingots made. Middlesbrough pigs are computed to contain about 1¼ per cent. of phosphorus. The phosphorus in the Staffordshire pigs, which have been most largely used, is about 3 per cent. With such pigs the results were obtained which were last week described in *The Engineer*. Since that time pigs in which the quantity of phosphorus is estimated at as high as 4½ per cent. have been blown. These, treated by Mr. Gilchrist with an extra proportion of lime, have made slabs and billets deemed by that inventor to be in no way inferior to those resulting from the use of pigs with 3 per cent. of phosphorus. Arrangements have been made for completely testing all the slabs and billets. Eighteen firms are now receiving lots of from two or three to five tons apiece. Treated in the ordinary iron mill, these slabs will be rolled out as if they were piles made of puddled iron or scrap, and the sheet or strip, or what-not, will be experimented with by the stampers, the tin-plate makers, the tube makers, and the rest. Upon the reports of the testing firms will largely depend the adoption of the basic Bessemer process in districts where common pigs are abundant but high qualities of hematite pigs scarce.—*Engineer*.

MANUFACTURE OF STEEL AND INGOT IRON FROM PHOSPHORIC PIG IRON.—At the Society of Arts, in April last, a paper was read by Sidney Gilchrist Thomas and Percy C. Gilchrist on the manufacture of steel and ingot iron from phosphoric pig iron. The authors, after stating that nearly nine-tenths of the iron ores of Europe were so phosphoric as to produce a pig iron unfit for steel-making without a process of dephosphorization, showed that by the new lime process perfect dephosphorization was produced so that the steel made from phosphoric pig was actually purer than that made from hematite iron. They then instituted a comparison between the basic Bessemer process and the puddling process, pointing out that the former process was peculiarly adapted to the manufacture of soft weldable steel, having all the characteristics of puddled iron with considerably greater strength, elasticity, and ductility. It was stated that this soft basic Bessemer steel could be made for some shillings a ton less than ordinary puddled iron, while an economy of 7s. a ton was gained in its subsequent treatment by the smaller loss which it undergoes in rolling. The authors stated that nearly half a million tons a year of the new dephosphorized metal were now being made, and that on the Continent works were erecting, having a capacity of a further half million tons a year, while in England the new special works erecting had only a capacity of under 200,000 tons a year. The paper concluded by querrying the wisdom of allowing Continental ironmasters to push so far ahead of us in the production of this new ingot iron, which was not only cheaper but immensely superior to puddled iron.—*London Paper.*

SELF-WINDING CLOCK.—In September last, a new perpetual clock was put up at the Gare du Nord, Brussels, in such a position as to be fully exposed to the influence of wind and weather; and although it has not since been touched, it has continued to keep good time ever since. The weight is kept constantly wound up by a fan, placed in a chimney. As soon as it approaches the extreme height of its course, it actuates a brake, which stops the fan; and the greater the tendency of the fan to revolve, so much the more strongly does the brake act to prevent it. A simple pawl arrangement prevents a down draught from exerting any effect. There is no necessity for a fire, as the natural draught of a chimney or pipe is sufficient; and if the clock is placed out of doors, all that is required is to place above it a pipe, 16 or 20 feet high. The clock is usually made to work for 24 hours after being wound up, so as to provide for any temporary stoppage; but by the addition of a wheel or two, it may be made to go for eight days after cessation of winding. The inventor, M. Auguste Dardenne, a native of Belgium, showed his original model at the Paris Exhibition of 1878; but has since considerably improved upon it.

PURE CARBONS FOR THE ELECTRIC LIGHT.—At the meeting of the Paris Academy of Science on 27th March, M. Jacquelain pointed out that carbon for the electric light should be purer than that obtained by calcining wood; and, if not free from hydrogen, should, at any rate, contain no mineral impurities. There are three methods for accomplishing this result: (1) By the action of a jet of dry chlorine gas directed on the carbon, raised to a light red heat; (2) by the action of potash and caustic soda in fusion; and (3) by the action of hydrofluoric acid on the finished carbons. M. Jacquelain has prepared carbons by all three methods, and has summed up, in a table, the photometric results of his experiments. He comes to the conclusion that the luminous power and the regularity of the voltaic arc increase in direct ratio to the density, hardness, and purity of the carbons. He remarked, incidentally, that the natural graphitoïd of Siberia possesses the singular and unexpected property of acquiring, by purification, a luminous capacity double that which it has in the natural state, and which exceeds by one-sixth that of pure artificial carbons.

BOOK NOTICES

PUBLICATIONS RECEIVED.

PROCEEDINGS OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

JOURNAL OF THE ASSOCIATION OF ENGINEERING SOCIETIES.

ABSTRACTS OF FOREIGN TRANSACTIONS, PREPARED FOR INSTITUTION OF CIVIL ENGINEERS.

HOUSEHOLD CHEMISTRY FOR THE NON-CHEMICAL. By A. J. Shelton, F.C S. London: F. V. White & Co.

This is a work which strongly reminds us of the late Prof. Johnston's "Chemistry of Common Life." The writer, indeed, admits in his preface that many books have been written on the chemistry of things commonly met with in daily life, but contends that they have been at fault "in at least one particular," *i.e.*, in containing a quantity of matter "not of a strictly chemical nature, and which, however interesting in itself, swells the book to a large size without adding to its usefulness." It might perhaps here be remarked that matter not strictly chemical may yet be very useful, and may be legitimately introduced into works of a popular class. Indeed, in describing, as the author proposes to do, "certain chemical principles and processes involved in some household operations," it will not always be found easy to eliminate physical and physiological considerations.

Mr. Shilton devotes his first chapter to "chemical preliminaries." In the second he treats of washing soda, common salt, and other sodium compounds, and describes briefly the alkali manufacture. The manufactures of soap and of candles are next sketched. As regards the latter subject it may be asked whether, as the processes of candle-making are mainly mechanical, the author is not, like his predecessors, introducing matter which is "not of a strictly chemical nature."

Ozone, though it figures as an item on the

cover of the book, is but slightly noticed in the text. We are glad to find that the author shows himself sceptical as to the alleged wonderful powers ascribed to this compound. He is even hard-hearted enough to inform the British public that the peculiar odor which they greedily inhale at the seaside and regard as a panacea consists principally of the effluvium of "decomposing crabs and seaweed." As regards the proportion of carbonic acid in the air, the chief weight is still laid, as in older manuals, upon its production by the respiration of animals and by combustion, and on its decomposition in the nutrition of plants. But we find no mention of a pair of processes which are at work on a probably larger scale, *i.e.*, on the one hand the exhalation of carbonic acid from volcanoes, and on the other, its withdrawal from the atmosphere in the form of calcium carbonate by certain processes of marine animal life, especially by the coral worms.

The chapter on water contains some very sound advice, and we are glad to perceive that the author gives his vote for soft water as the more suitable for domestic purposes. The cost of softening a hard water by dint of soap is £47 1s. 8d., as against 8d. for doing the same work by Clark's process. A section on disinfectants, though correct in its statements, does little more than show how very limited as yet is man's power of dealing with disease germs.

Succeeding chapters deal with starch, the sugars, the manufacture of bread, though without any reference to the ultra-filthiness of our modern town bakeries, fermentation, distilling, wines, where the "plastering" fraud is duly denounced, vinegar, the infused beverages, the glass and porcelain manufactures, and the chemistry of food. As the entire compass of the book falls short of 200 pages, not very closely printed, it need scarcely be said that these subjects can be but briefly dealt with. The author, however, may fairly be said to have made the best of his narrow space, and to have given a clear summary of his subjects.

DYEING AND TISSUE PRINTING. By W. Crookes, F. R. S. (TECHNOLOGICAL HANDBOOKS.) Edited by H. Trueman Wood, Secretary of the Society of Arts.) London: G. Bell and Sons.

Those of our readears who have taken an interest in the City and Guilds of London Institute for the Advancement of Technical Education will be aware that the want of a series of manuals specially adapted for the use of students preparing for the examinations of the Institute soon made itself felt. On the tinctorial arts, for instance, there certainly existed important and valuable treatises. But they were for the most part too costly for students, many of whom would probably be of limited means. Other works, again, were unsuitable because they did not begin at the alphabet of arts in question. What was needed, therefore, was a handbook, not too costly, plain, and simple in its style, covering the whole ground, and making no special demands upon the previous knowledge of the student. Mr. Crookes has undertaken the somewhat difficult task of drawing up such a work, and appears to have succeeded in fulfilling the various conditions above laid down. The only previous qualification of which the student is assumed to be possessed is an elementary knowledge of chemistry, such as may be acquired from almost any of the rudimentary treatises on that science. The author, building upon this foundation, seeks to explain the principles of the art from a practical rather than from a theoretical point of view. From the very outset he endeavors to explain everything with which the learner might be puzzled. In the preface there are given explanations of certain measures used in dye-works, &c., and little known elsewhere. In the "General Introduction" the first point brought forward is the cleansing of the goods to be operated upon—a matter in which even experienced dyers are often sadly indifferent, and thus insure an unsuspected source of blunders, which are charged against the dye-wares or the mordants, and which can often be rectified only by the expenditure of much time and trouble. Mr. Crookes even demands, as far as is humanly possible, chemical purity in the vessels used, in the materials to be dyed, in the water, and in the dye-wares. We know that good results are often produced without the observance of these conditions, but we know also that a prudent man will, if possible, avoid the risk. Half the skill employed in "cobbling" pieces which have come up spotty, or flat, or smeary, would have prevented these evils, and given a far better result.

At this part of the treatise a description is given of the procedures for bleaching the different textile fibers, that is, freeing them from their natural coloring-matters, which in many cases if let remain would be as fatal as artificial dirt.

The next section, on the selection of water for dye and print-works, has been evidently written with great care. The author points out what kinds of water are needed, from what geological formations it may best be obtained, and what possible ingredients are to be especially avoided. It may here be remarked that the water needed for tinctorial purposes, and, indeed, for the industrial arts generally, is not the same quality as that which sanitary reformers demand for domestic purposes. For dietetic purposes the presence of salts of lime, and even of magnesia and iron, to a moderate extent, is not objected to. For the dyer or the printer, iron is fatal, and compounds of calcium and magnesium greatly interfere with many of his operations. Processes are given for the detection of the ordinary impurities, and for their removal, when necessary, upon the large scale.

Next follows a chapter on mordants. Here the author enters a little more into theoretical considerations than in most parts of the work. He shows that if the action of the metallic mordants and the nature of the aniline colors had been better understood, practical men might have been saved the trouble of tedious attempts to fasten, *e. g.*, magenta upon cotton fiber by means of aluminium acetate or sulphate. Surely, even those who talk most loudly of the

uselessness of what they are pleased to brand as mere "book-knowledge," might see the necessity of having some acquaintance with the properties of the agents they use. To argue that because magenta is a red color it must be capable of fixation in the same manner as cochineal, is not, after all, a very practical procedure. The instructions for the preparation of nitrate of iron rank among the fullest which have ever been printed, and speak of close and extensive observation.

The accounts of the astringents, of the fatty and the animal mordants—commonly so-called—are exceedingly thorough going.

In the "General Instructions on Dyeing" we find not a little matter which it is probable has never appeared in print before, having probably been overlooked as too elementary. Among other needful matter we find here the introduction of certain technical terms, which would greatly perplex the tyro on his introduction to practical work. Here, also, are plain directions for "matching off" colors, *i.e.*, for comparing the goods dyed with the pattern sent as a standard.

After these general and introductory considerations, follow a series of receipts for obtaining different colors upon cotton. It has evidently been the author's object to exemplify the methods required for dealing with cotton in its different states, such as cotton-wool, yarns, piece-goods of various kinds, such as calico, cotton-velvets, cords, &c., and to show the processes for applying the new colors.

After cotton, linen, jute, wool, and silk, are worked through in a similar style, the characteristic features of each staple being noticed in a few preliminary remarks.

The latter half of the book is devoted to tissue-printing in its various styles and branches. It cannot be denied that the work would have been more useful had it been illustrated with dyed and printed patterns, diagrams of machinery, &c. But such additions would have involved such an increase in the price of the book as to be out of the question. For the purpose in view this treastise will form a sound and useful basis for the student.—*Chemical Review.*

MISCELLANEOUS.

In the Belgian Academy, M. Plateau has lately called attention to a small illusion. He describes an arrangement, which, at first sight, he says, might be thought capable of realizing perpetual motion. A capillary tube is inserted obliquely in distilled water, so that the latter nearly fills it. Into this liquid column, at the top, dips the small orifice of another tube, which reaches a little way in the same oblique direction, then turns downwards, the vertical portion being wider, and not reaching the water. Suppose this bent tube filled with water. It then forms a siphon, the shorter branch of which is immersed in a liquid in equilibrium, while the longer descends several centimeters below the surface of that liquid. Does it not appear as though the water should flow incesantly through the siphon, and, regaining the vessel, be engaged in perpetual circulation? As a matter of fact, the water is drawn upwards in the vertical portion of tube till its free surface reaches a part of the oblique part of the same tube, when it stops. M. Plateau accounts for the effects by suction exerted by the small concave liquid surface between the two tubes.

The fourth number of the *Memoirs* of the Science Department of the University of Tokio is a monograph on the geology of the environs of Tokio, by Prof. Brauns; while the fifth contains a paper by Prof. Mendenhall on the force of gravity at Tokio and on the summit of Fujiyama. Dr. Naumann, the head of the Japanese Geological Survey, has recently published a monograph on Japanese elephants. The writer has found remains of these mammals in various widely separated districts. This paper will be found in vol. xxviii. of the "Palæontographica," published by Fischer of Cassel, and is entitled "Ueber Japanische Elephanten der Vorzeit."

An alleged invention of a German chemist, by which cotton and woolen fabrics could be coated with a layer of dissolved silk and made to assume the glossy and soft appearance of actual silk goods, was recently described by the *Colonies and India*. Experiments in a somewhat similar direction appear to have been made by a French chemist, who, however, coats his material with a thin layer of tin instead of silk. He first makes a mixture of zinc powder and dissolved albumen, which he spreads over the fabric by means of a brush, leaving it to dry, when the stuff is passed first through superheated steam, and afterwards through a solution of chloride of tin. By this means an exceedingly thin layer of tin is spread over the whole side of the fabric, which is thus rendered waterproof, and protected against erdinary rough usage. The utility of the invention probably consists in the preparation of theatrical dresses, and even in the bright "trimmings" the invention might find a limited application.

Stannous hydrate may lose its water and become transformed into crystals of the anhydrous oxide under circumstances which are complex and imperfectly known. The crystallization may occur either in acid or alkaline liquids. The acids with reference to oxide of tin may be divided into two groups. Those of the one group give, with this oxide, salts which are entirely decomposed by boiling water, and determine its transformation into the crystalline oxide in consequence of successive reactions. These salts, decomposable by water, yield, free acid, and behave absolutely like the acids themselves, determining the crystallization of stannous oxide. The acids of the second class do not give rise to these successive reactions, and the hydrated stannous oxide never becomes anhydrous and crystalline under their influence.